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Mixture as before—to 1984?

IN A week's time the British electorate will instal a new government which could, if returned with a sufficient majority, see the nation through to 1984. The three major political parties, Labour, Conservative and Liberal, each publish brief but comprehensive manifestos regrettably padded out with a tedious mixture of boasts and insults. Somewhere in their midst, however, we ought to be able to discern clear, separate strands of political thinking which differentiate the parties and which when viewed against the current economic and social background give a clear indication of the most appropriate party for the next five years.

What is this background? In the 1970s Britain's industrial production has barely risen at all, whilst that of other major industrialised countries has climbed—in difficult circumstances—by amounts ranging from 15% to 30%. In terms of gross domestic product per capita, 15 years ago Britain was closely on a par with many European countries and nearly a factor three ahead of Japan. Now most of Europe has left us far behind, and Japan is fast doing so too. Only in the inflation league has Britain shown its competitors (with the exception of Italy) a clean pair of heels. All of this has happened to a country not notably short of material and energy resources, and certainly not lacking in human and intellectual resources.

Many argue, quite reasonably, that Britain is still an agreeable country in which to live, that the people are civilised and not driven by the materialist urges that allegedly are rife in other countries. Why, it is said, worry about international growth, productivity and efficiency leagues in such a green and pleasant land? The answer to this must surely be that unless we exert ourselves to the full, competition from other countries will not allow us just to stand still—it will rapidly bring on irreversible decline and the emigration of large numbers of talented people.

With the coming of post-industrial society, in which a diminishing number of people are involved in factory-style production and a growing number in some sort of 'information activity'—teaching, research, entertainment, printing, health, journalism, banking and so on, it is particularly important that flexibility be emphasised. It is almost impossible to envisage what the world will be like in 10 years' time, but one thing is certain: that it will have undergone rapid change, and that unless strong action is taken, a lot of people—indeed whole nations—are going to be left behind.

The strong action must take two forms—first, to permit or even encourage declining industries to die peacefully without deceiving those associated with them into thinking that better times are just around the corner; second, to take much more vigorous steps to promote lifelong education and retraining, so that going back to school or college is seen not as a humiliating thing of last resort, but as a perfectly natural thing to do.

How much of this is hoisted in by the two main political parties—and how much are they prepared to act on it? The Conservatives are probably more capable than Labour of being ruthless with declining industries. But they do not show much sign of recognising the changes that need to be wrought in our educational system beyond stating "we shall review the relationship between school, further education and training to see how better use can be made of existing resources"—which could mean anything. On the whole Labour seems, in its manifesto, to appreciate the changes that are necessary—even putting in a reference to the impact of the silicon chip—but the party has largely presided over the years of industrial decline without doing much to arrest it.

The real source of Britain's dilatory industrial performance cannot be blamed on a scarcity of resources, nor on a lack of intellectual skills. It must be placed squarely on our apparent inability to work together without breaking up into isolated groupings—'workers', 'skilled workers', 'management', 'civil servants', 'academics' and so on, each finding difficulty in understanding the other and each either busy exploiting the others or accusing the others of exploitation. To the extent that the British are naturally sceptical of authority, lack of homogeneity is fine. But the divisions are much more bitter and unproductive than that. Both major political parties exist, indeed thrive on these dichotomies, and so are themselves part of the problem they should be trying to eliminate. This, then, should make the Liberal party an attractive alternative. Certainly the Liberal manifesto touches on many of the matters discussed above, and the party even makes a play for being regarded as the environmental party. Traditionally a vote for the Liberals has been regarded as wasted in the British system of representation. That must not be allowed to deter thoughtful people, worried about an ever-declining, ever-confrontational Britain, from looking very seriously at the Liberals. □



University challenged over 'agribusiness' connections

David Dickson contributes his third weekly article on science in the American West with a report on farm workers in California, who are suing university officials over the social consequences of agricultural research

EARLY next month a county court in California will begin hearings on charges that have been brought against the University of California, claiming that it has carried out agricultural research leading to a variety of undesirable social consequences. These include the disappearance of harvest-time jobs as a result of increased automation, the shift from small family farms to large-scale 'agribusinesses', and the enrichment of corporate stock-holders out of public pockets.

A suit filed by a publically-financed group, California Rural Legal Assistance, on behalf of a number of farmworkers, concentrates in particular on the way that university research has contributed to the growth of mechanised agriculture in the state, resulting, the group claims, in extensive job dislocation and unemployment.

The suit demands that such research be stopped. And it also accuses senior members of the university and its Board of Regents of a conflict of interest over their connections with large food-producing corporations which, it is claimed, have been the principal beneficiaries of state- and federally-financed research.

The university, in response, has strongly denied any impropriety. In a statement issued at the end of last week, university counsel Mr Donald C. Reidhaar said that if the suit succeeded, it would prevent the university from carrying out research which might result in harm to certain people and in help to others.

"A basic mission of the university is research and the creation of new knowledge. Acceptance of the plaintiff's proposition would require the elimination of all research with any potential practical application," Mr Reidhaar said.

Research workers at the University of California have been engaged for more than a hundred years in developing techniques to raise agricultural production; ironically one of the university's first major political crises involved the claim that it was not doing enough of practical assistance to local agriculture.

Much of this research has been related to mechanising traditional agricultural practices, often with state support. In the mid-1960s, for example, the university was awarded an annual grant of \$150,000 by the state legisla-

ture to carry out research into automated farm machinery, following the phasing-out of a programme to bring farm labourers across the border from Mexico at peak harvesting time.

Yet not everyone has appreciated—or benefitted from—these changes. In tomato growing, for example, research at the university's Davis campus has led to developments ranging from automated picking and sorting machines, to a strain of "square tomato" whose thick skin protects it during mechanical processing; but many small farmers who could not afford the expensive new equipment have been forced out of business, while tomato harvesting no longer provides summer-time work for many thousands of migratory workers.

Strong opposition to the increasing automation of agriculture in California—as well as to the university's role in encouraging this process—has come from the United Farm Workers, the union established in the early 1970s after several bloody confrontations with growers. The union is concerned, for example, that many farm owners have used the elimination of jobs to offset the increased wages that the union has been able to negotiate for its members.

"We are not against mechanisation, but do not think that the taxpayer should pay twice, first for the univer-

sity research and then for the increased unemployment, welfare and social service costs" says union official Mack Lyons. Last year the union's convention passed a resolution demanding a moratorium on future research in agricultural mechanisation until it had been assured that the interests of farm workers would be protected.

The legal challenge, however, has come not from the union, but from a group of attorneys working for an organisation set up on state and federal funds to protect the rights of farm workers. In a suit filed in the Alameda County Court, it is demanded that the university stop all research on any agricultural mechanisation process that conveys has "a special economic benefit to narrow, private agribusiness interests at the expense of farmworkers, small family farmers, consumers, taxpayers and the quality of rural life".

By using tax money to benefit a small group of private corporations and by acting out of individual economic self-interest, the plaintiffs claim that the university and various named members of its Board of Regents have acted contrary to the state constitution, which requires that the university be administered as a public trust for the benefit of all Californians, free of political influence.

"We are questioning who benefits from the technological changes resulting from this research, who gets hurt—and who takes the decisions. In this type of situation we feel that there should be an open decision-making pro-



"The machine won't strike, it will work when the growers want it to work"

cess before money is spent on research," says Mr Al H. Meyerhoff, one of the attorneys who have filed the suit on behalf of nineteen farmworkers and the California Agrarian Action Project.

"The increasing monopoly over agricultural production of large food-producing corporations is being facilitated by research and development work at public universities initially established, under the land grants colleges legislation, to help small farmers. We feel these institutions should not be contributing to the problems that these farmers face."

The university strongly denies charges that the results of its agricultural research programmes have been socially detrimental. In particular, according to university staff:

- although mechanisation has resulted in declining employment in some areas, this has been largely compensated for both by the introduction of new jobs in other areas, and by other employment opportunities brought about by general increases in agricultural productivity;

- rather than merely benefitting large corporate producers, the technological developments arising from university research have been of general benefit to the community, the advantages of increased productivity for example being passed on through lower food prices;

- and the university is also challenging whether it should have any particular responsibility for the social consequences of its research programmes, or whether this responsibility should not be shared by the whole community.

"The university's responsibility is to create new knowledge or information, to develop new ways to produce food as efficiently as possible, and to be aware of new developments, and so forth. But in terms of the conflict of social goals, that's not only *our* job, but the job of society, of the legislature," Professor Charles Hess, dean of the college of agricultural and environmental sciences, said in a recent interview.

Others at the university strongly support this view, although many admit that the automated machinery that they have developed has been made particularly attractive to local growers by the increasing strength and militant tactics of unionised farmworkers.

"Automatic lettuce harvesters, for example, developed at the university have been available for some time, but have not been widely taken up for a number of reasons, in particular cost. But I don't know how many more lettuce strikes we will have before something happens," says Dr William Chancellor, professor of agricultural engineering at the university of Cali-

fornia's Davis campus.

From the union's standpoint, increasing mechanisation is a direct threat to its bargaining capabilities. One tomato grower near Sacramento, the target of an unsuccessful UFW campaign in 1975, subsequently bought an electronic tomato sorter for \$200,000, and was able to reduce his work force from 100 to 28, thus getting rid of "all the troublemakers". As one university staff member has been quoted as saying, "the machine won't strike, it will work when the growers want it to work".

The farmworkers have already received considerable support in their fight against mechanisation from members of the state legislature. At the request of one state representative, for example, the state accounting office is already carrying out an audit of the university's research activities to see if it reveals any "improprieties".

But neither has the university been totally insensitive to its criticisms. In addition to publicising the social value of its research, the university points out that the amount of research into agricultural mechanisation is being decreased, with emphasis shifting, for example, to methods for improving the biological productivity of crops.

The university is also both carrying out research and offering retraining courses aimed at the problems faced by farmworkers who lose their jobs as a result of automation. "We have been accused about not caring about the problems that mechanisation causes; but we are now looking at these too," says Dr Chancellor.

In responding to the charges made by the legal aid group, however, the university has denied that there is anything improper in the close links that it has established with private industry; claims that such links result in an "inordinate influence" on research policy are, it says, subjective assessments based on a particular political viewpoint.

Critics remain unconvinced. They blame the major food producers for the social problems of US agricultural workers—as well as the declining flavour of US food—and accuse the University of California (as well as universities playing similar roles in other states) of direct collaboration in this process.

"It belongs to society as a whole to decide what help people affected by agricultural developments should get, and how much. We should not be expected to do this on our own," says one university spokesman. "We believe that it is a travesty for the government to use tax money, in the form of research grants, to force people out of work and drive small family farmers off the land," says Mr. Meyerhoff. □

Professional bodies lobby to protect US science budget

QUOTING a 25% drop in the proportion of the federal budget devoted to basic research between 1968 and 1978, 40 US scientific societies and higher education associations last week issued a joint statement supporting President Carter's bid for a significant increase in support for basic research in the fiscal year 1980.

The statement is critical of the administration's decision to request virtually no increase in funding for biomedical research through the National Institutes of Health, pointing out that this will mean a decrease of almost 50% in the number of new competitive research grants available.

Apart from this, however, the various organisations put their voices solidly behind President Carter's request for a 9% increase in basic research funding—even accepting that this will be barely sufficient to keep up with inflation—and urges Congress to do the same.

So far, the Congressional response to the budget request submitted in January has been relatively good. The Senate budget committee, for example, having taken a detailed look at the requested science budget, has recommended that it be accepted almost in full, although suggesting cuts in virtually all other areas of public spending.

But there may well be stormy weather ahead. The House of Representatives, for example, in authorising a budget for the National Science Foundation close to the \$1,000 million requested, accepted by 219 votes to 174 an amendment reducing funds for biological, behavioural and social sciences research (and aimed primarily at the last of these) by \$14 million; last year a comparable amendment was rejected 174 to 229.

Immediate cause for concern are imminent floor debates on broad budget resolutions in both the Senate and the House, with various proposals that could affect science funding. A further test will come when key appropriations subcommittees meet to decide on agency budgets later next month.

Keen to prevent a repeat of last year, when a substantial increase in funding for basic research requested by President Carter was cut back by Congressional committees to a level—apart from the NIH—scarcely above inflation, the research community has been busy putting its lobbying act together in Washington.

In issuing a joint statement, the

various higher education, scientific and technological societies said they were taking an "unprecedented step" in presenting to Congress a unified position on the needs of science. They expressed concern at "the lack of a congressional policy in science which would embrace the principles of stable, balanced and controlled investment in basic research".

At a press conference in Washington to coincide with the publication of the joint statement, Dr Derek Bok, president of Harvard University, said that there was no contradiction between President Carter's aims to achieve a balanced budget, and to increase the federal investment in basic research.

"These positions are inextricably related, for out of basic research, if history is any judge, will evolve industrial innovation and growth, more efficient and thus less costly uses of labour, energy and equipment, and better health and security for all," Dr Bok said.

Dr David Saxon, president of the University of California, pointed to the present energy crisis as a "very real example" of what could happen if investment in basic research was allowed to fall behind. "Congress used

foresight in 1954 when it amended that Atomic Energy Act to provide for research and development of alternative energy sources. However a review of the funding record for programmes of basic research reveals a history of peaks and valleys. This lack of sustained commitment has discouraged outstanding talent and blunted national progress."

The need to maintain stability in funding is, in particular, being used to justify demands for further increases in funding for the National Institutes of Health. In announcing that it was requesting a level of funding virtually the same as the current year (the Office of Management and Budget is even said to have suggested a decrease) the administration pointed out that Congress voted last year for a 23% increase in biomedical research support—and that even if this was spread over two years, it would still be comparable to planned increases in other areas over the same period.

Supporters of more funds for biomedical research, however, argue that things are not that simple, and that a stationary NIH budget would introduce instabilities by drastically cutting back on new research starts. "Analysis of

the budget by mechanism reveals a disconcerting and unacceptable reduction in the funds available for young investigators," says Dr Suzanne Oparil of the University of Alabama, chairwoman of the American Federation for Clinical Research.

A marked increase in obligated non-competing research projects supported by NIH would, she says, result in a 43% decrease in funds available for competing research projects. "Because young biomedical investigators are supported primarily by investigator-initiated competing research grants, this reduction in funds for competing grants must be vigorously opposed."

Biomedical research workers are particularly concerned that although in previous years Congressional committees have traditionally provided significant increases to medical research, the current mood against further public spending—together with the loss of several key Congressmen, such as Senator Edward Brooke and Representative Paul Rogers, who had previously championed the medical research cause—mean that a comparable increase this year is far from guaranteed.

David Dickson

US accused of banning foreign scientists

FOREIGN scientists invited to lecture or participate in scientific meetings in the US continue on occasion to suffer the embarrassment of being denied entry to a country "proud of its right to freedom of speech", according to a senior member of the American Association for the Advancement of Science.

This situation arises because those taking part in scholarly and cultural exchanges whose political beliefs fall into particular categories—and in particular past or present members of the Communist party—must obtain a special waiver from the State Department to obtain a temporary entry visa.

"In general this policy has been implemented in a liberal way so as to remove automatic exclusion of foreign scientists and others who might express beliefs or support for political doctrines previously restricted by our immigration laws," Professor John Edsall, chairman of the Committee on Scientific Freedom and Responsibility, told a Congressional committee studying US compliance with the Helsinki Agreement earlier this month.

"However in some cases the policy has remained unchanged, or foreign visitors have been excluded on grounds other than political beliefs, thus making the waiver procedure more difficult to implement. What is most desirable from the view of scientists is the removal of these restrictions from

the US immigration laws."

Professor Edsall, who is professor emeritus of biochemistry at Harvard University, listed a number of cases which, he said, appeared to violate the principles of free travel as stated at Helsinki, instances in which political or administrative decisions had impeded the free circulation of scientists. These included:

- The refusal of a visa application—later granted—to Dr Sylvia Berman, an Argentine psychologist now living in exile in Mexico city who is alleged to have previously expressed Marxist beliefs, to participate in a meeting of the American Public Health Association in Washington DC, last year.

- The denial of a visa in 1975 to Dr Joseph Needham, the British science historian. Although the state Department informally acknowledged that this was because of his allegedly Marxist beliefs, it refused to provide him with the reason for the denial. Dr Needham subsequently refused to apply for a waiver. On subsequent visits, Dr Needham has been granted a visa without having to apply for a waiver.

- Late last year, delays in reviewing the visa application of Professor Jean Pierre Vigier, a French physicist who had been invited to address the AAAS annual meeting in Houston, Texas, made it impossible for him to attend the conference.

- Dr Andre Frank, professor in the

school of development studies at the University of East Anglia, although previously a US resident, has consistently been denied a visa on various grounds, both regarding his political affiliations and unspecified evidence—which Dr Frank denies—that he may wish to take up residence in the US again.

Professor Edsall said that other areas of concern involved the controls placed on the travels of foreign scientists while visiting the US, since the State Department has divided the country into open and closed areas as part of our diplomatic relations with the Soviet Union and East European countries.

"It appears questionable at the present time whether such a travel restriction policy will also be applied to the Chinese scientists, who are currently seeking expanded scientific exchange with the US. In the interests of science it is important to promote free access to all individuals who wish to exchange information, without arbitrarily assigning the need for screening or political controls", he said.

"Our violations are, I would agree, a good deal less serious than those that have occurred and still occur, in countries with Communist governments; but let us do away with them and show that we truly honour the spirit of the Helsinki accords."

David Dickson

Soviets boost their own...

LAST week, the Soviet Union celebrated its new "Science Day" (the third Sunday in April). To mark the occasion, *Pravda* published an extensive article by Academician Anatolii P. Aleksandrov, President of the Academy of Sciences, which, while praising, naturally, the achievement of Soviet science, did so largely at the expense of the West.

The new Soviet "Science Day", of course, has considerable political overtones. The date was chosen to commemorate 12 April, 1918, when the Soviet of People's Commissars directed the Academy of Sciences to solve, as an "important and urgent" problem, the "correct distribution of industry in the country and the best rational use of its economic forces".

Accordingly while tracing the source of Soviet science from its early days to the present work-force of 1.3 million scientists ("the largest scientific team in the world—its potentials are enormous") Aleksandrov found it necessary to devote almost a sixth of the article to the Cold-War.

Partly, it is clear, he wanted to exculpate Soviet scientists involved in armaments production.

Then, after a brief paragraph on Brezhnev's peace initiatives, Aleksandrov moved the attack to the economic front. The Soviet Union "carrying out Lenin's directives has created a reliable fuel-energy basis of its own, a diverse basis of raw materials, and all forms of industry. . . . "There is no scientific-technical problem" he concludes triumphantly "that would not be within our power".

Imports of such technological goods as gas pipelines, he explains, are simply a way of receiving payment for exports of "excess production", and allow "advances in the necessary sectors" to be made; they are never, he averred, "vitally necessary".

To ram his point home, he then described the US economy, on the brink of energy shortages and major economic crisis, following this by a review of the benefits of Soviet science, "not only for our country, but for mankind as a whole"—the first nuclear power station, the first nuclear ice-breaker, the start of space research. Other Soviet successes, he maintained, had "given the impetus for corresponding work to be done in the West"—lasers, Cerenkov radiation, "certain principles of thermonuclear synthesis", and "the production of relativistic electrons".

The final column is quieter in tone; an allusion is made to Mr Brezhnev's enthusiasm for basic research, the instrument-construction and computer sectors receive a slight censure, the RATAN-600 telescope and Baksansk neutrino observatory are particularly praised.

And that is all. The feast-day of Soviet science, as portrayed by Aleksandrov, is an entirely Soviet celebration. The outside world is cited only to make the Soviet picture shine more brightly by contrast.

Perhaps most significant, although the politicians regularly mention scientific and technical cooperation as one of the benefits of the Helsinki accords, Aleksandrov does not reciprocate the compliment. In what must, by its length and occasion, count as a major statement on Soviet science policy, the word "detente" is never mentioned.

... but bring the Bulgarians down with a bump

ABORTING the Soyuz-33 flight with the Soviet and Bulgarian cosmonauts must have proved particularly embarrassing to the Soviet space planners. Not only was it a premature return, but it took place on "Cosmonautics Day" (12 April, the anniversary of Gagarin's flight).

Bulgaria's space research programme has always been particularly closely linked to that of the Soviet Union. It was initiated in 1956 following a Soviet suggestion that the Bulgarians should explore the possibilities of carrying out ionospheric research and monitoring future Soviet satellites. The Bulgarian Council of Ministers established a Centre for Radio-Observation and Ionospheric Research, which was ready to go into operation when Sputnik-1 was launched in October 1957.

At the same time, other Comecon countries—Czechoslovakia, Poland, East Germany and Hungary, were making a firm commitment to space research. This, however, was a stepping

up of existing efforts, not, as in Bulgaria's case, an entirely new initiative.

By 1966, when the *Interkosmos* programme was initiated, Bulgaria had caught up considerably. From the beginning, there was a major Bulgarian participation in the *Interkosmos* working groups on space physics, satellite communications, space meteorology and space biology. On the hardware side, Bulgaria was still somewhat behind although *Interkosmos-2* included a joint Soviet-Bulgarian Langmuir probe experiment. By December, 1972, Bulgaria was able to provide a complete experiment, monitoring electronic and ionic concentrations and temperatures. Updated versions were later carried by the *Interkosmos-12* and -14 satellites, and the *Vertikal-4* and -5 geophysical rockets.

In addition, Bulgarian scientists have been involved in the equipping and processing of experiments carried by several Soviet *Kosmos* satellites. Throughout, Bulgaria's main interest has been directed towards particle and field distribution, including some interesting results on magnetosphere shock-waves. It is to such experiments that Bulgaria's own satellite, *Bulgaria-1300*, will largely be devoted.

This satellite is intended to celebrate the forthcoming 1300th anniversary of Bulgarian statehood, and will presumably engage the entire launch power of an *Interkosmos* blast-off, resembling the Polish *Copernicus-500* jubilee satellite of 1973, rather than the small Czechoslovak *Magion* satellite, which was put into orbit as part of *Interkosmos-18* last autumn.

Bulgaria would then become the second non-Soviet Comecon country to have a satellite of its own—which should compensate considerably for the loss of *amour-propre* over the *Soyuz-33* abort. Indeed, the Bulgarian media have already done an excellent PR job on *Soyuz-33*—backed up by the Soviet and Bulgarian governments which have awarded Ivanov and his Russian commander Rekvashnikov the usual orders and titles. The return to Earth, using the back-up engine for re-entry, and the reactions of the cosmonauts to this "short but difficult" flight were presented as valuable lessons in space dynamics and psychology.

As for the Bulgarian experiments designed for the mission—which were to include a unique spectroscopic survey of the Earth—these will almost certainly be carried out by a repeat mission in the near future. So, when the current programme of *Interkosmos* manned flights comes to an end, Bulgaria may well have two manned flights (and possibly two cosmonauts) to her credit, while the other fraternal allies have only one apiece.

Vera Rich



Ivanov: awarded the usual medals

Union steps up lab safety campaign

UNION action at UK university and public health laboratories which use dangerous pathogens is to be stepped up in further attempts to improve safety. The Association of Scientific Technical and Managerial Staffs (ASTMS) has warned that it is now to launch a campaign to enforce greater safety at laboratories dealing with category B pathogens—such as those responsible for botulism, typhoid, paratyphoid and the plague. These are organisms less deadly than the category A smallpox virus which caused the death of medical photographer Janet Parker at Birmingham University last year but they still pose special hazards for workers.

The general secretary of ASTMS, Mr Clive Jenkins, said last week that the union would be telling its members: "Do it cleanly. Do it safely. Wear masks when necessary. Use the autoclaves. Use the fume cupboards. Demand things be made safe from the beginning". And he added: "We still have a situation in about 60% of our laboratories where mouth-pipetting is still permitted. That is disgraceful, awful. We will certainly be urging our members to stop mouth-pipetting at these category B laboratories."

"I think heads of departments are being dirty, careless and slovenly. There is no excuse for doing things incompetently, inefficiently and in a dirty fashion".

Mr Jenkins said the Birmingham smallpox outbreak and the subsequent revelations of the Shooter report had spurred ASTMS to take a fresh look at the 29,000 people in the laboratory population of universities and the health service. "These people are being ravaged by disease and are contracting all sorts of things like brucellosis, psittacosis, beryllium contamination, TB, hepatitis, and about eight or nine other things as well", he added.

The union now has 2,000 legal cases pending on behalf of its members—of which about 60% are related to diseases with industrial or laboratory sources. More staff have been taken on to help cope with this load and ASTMS has also rejigged its entire legal department to be investigative rather than retaliatory, Mr Jenkins added. "Our members are flooding us with calls now they realise that we are passionately interested in this area", he claimed.

ASTMS is now negotiating a new pay package for its technicians and is expecting a 9% rise backdated to last



Clive Jenkins: mouth pipetting "is disgraceful"

October which is to be followed by three other increases over the next 18 months. But Mr Jenkins stressed that although this claim had led to several disputes at departments, the union was trying to rein in its members. "One can damage a university without having any effect at all upon politicians or upon the economy and therefore we feel guilty because in the private sector we have been getting quite sensible agreements. But the universities are a real problem for us".

"We want to abolish the concept of danger money", Mr Jenkins said. "The arguments that we would use are that this is a very hard-working, productive, active and well-qualified group. Discussions about disease are separate. University technicians should be properly rewarded for their qualifications and work but we are not going to be bought off for more money to have them working in slovenly laboratories".

At the same time, ASTMS is still pressing for a public inquiry into the Birmingham outbreak with a legally qualified chairman and some form of union representation. Indeed this latter approach is a particularly important one for the union which wants far greater union representation on all bodies administering laboratories.

And Mr Jenkins reacted bitterly to the suggestion reported in *Nature* (29 March, p. 387) of Mark Carlisle, Conservative education and science spokesman, that there should be no lay or union members on the Dangerous Pathogens Advisory Group, the body responsible for screening biological laboratories such as the one at Birmingham.

"I would have thought that that was a scandalously reactionary suggestion. DPAG has been a disaster. We want it disbanded. Everyone inside it should be sacked and the body reconstituted within the Health and Safety Executive. I don't know why he wants the Tories to be the party of germs and danger".

Robin McKie

UK taxonomy is healthy, says report

A REVIEW group of the UK Advisory Board for the Research Councils (ABRC) has recommended that the major support in Britain for taxonomy—the classification of plants and animals—should go to studies of organisms of economic and social importance. Most in need of study it says, are the nematodes, tiny and prolific worms that attack crops and livestock throughout the world. This reflects the concern of the European Science Foundation Group on Biological Recording, Systematics and Taxonomy, which in 1977 instituted a survey of European plant-parasitic nematodes.

The classification of plants and animals may not be one of the more glamorous branches of biology, but the accurate identification of species, both past and present, and the study of their relationships is basic to both fundamental and applied biological progress. Reawakened interest in the field has inspired the official ABRC report, *Taxonomy in Britain* (HMSO, £3.50). The report has been produced by the ABRC's Review Group on Taxonomy, set up in 1974 under the chairmanship of Sir Eric Smith, formerly director of the Laboratory of the Marine Biological Association of the United Kingdom.

The state of British taxonomy seems on the whole to be healthy, the report concludes, although the group would like to see some changes, particularly to ensure proper care and use of the scattered national collections of biological specimens, and to encourage young people to take up taxonomy.

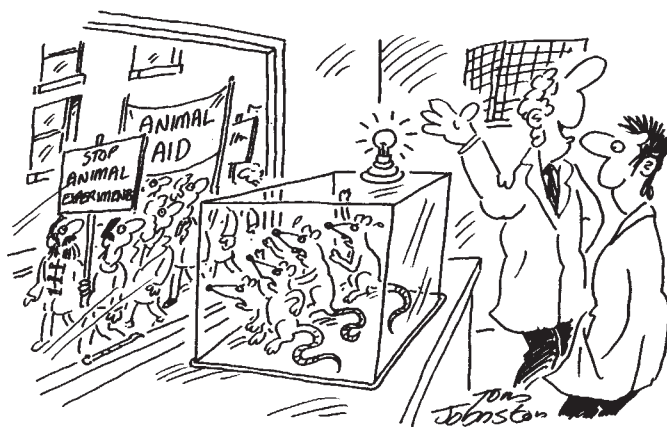
Also, the British collections of living material including, as well as the plants in the botanic gardens, cultures of algae, protozoa, yeast, bacteria, fungi and viruses, have special needs not shared by dried specimens. The report expresses particular concern about uncertainties in the funding of the collection of living fungi at the Commonwealth Mycological Institute and of the National Collection of Yeast Cultures and recommends that the ABRC review the funding of all such taxonomic reference collections.

To encourage recruitment and training of taxonomists, the report recommends that the study of taxonomy should be fostered as part of the general educational exposure to science and natural history. Field courses are vital, says the report. To build on these foundations there should be a modest increase in the small number of postgraduate studentships for taxonomic research.

Mary Lindley

news in brief

Cambridge animal demonstration: Protestors against the use of animals in scientific experiments will march through Cambridge, UK, on Saturday 5 May. The march has been organised by *Animal Aid*, and a spokesman for this organisation, Jean Pink, said their aim was to inform people about the 'horrific' and 'barbarous' experiments which were taking place in laboratories throughout the country. She said they picked Cambridge as the march venue because it was "the mecca of the scientific world and we want scientists to recognise our disapproval". *Animal Aid* want to see the end of all "cruel experiments" such as "depriving baby monkeys of their mothers at a very young age, and poisoning rabbits with chemicals" but they offer no alternative methods for scientists involved in this work. Marchers will assemble at Parker's Piece near the University Arms Hotel at 1.30 pm on 5 May and then march through the city centre. A meeting will take place afterwards at St Michael's Hall, Trinity Street, or on Parker's Piece if there are too many people. *Animal Aid* are expecting at least 300 people to attend the march. They have activist groups starting up all over the country, and may next demonstrate in Oxford and Reading.



"Every time the green light comes on they jeer and boo!"

New study shows cadmium danger in Shiphham: A new study (*Lancet*, 21 April), sharply disagreeing with previous assessments, has shown clinical symptoms of kidney damage in Shiphham residents exposed to cadmium from old mining tailings. Following the initial discovery of high environmental levels of cadmium (*Nature*, 25 January) Department of Health announcements and a pilot study headed by T. C. Harvey of the Department of Medicine at Birmingham University drew the conclusion that the situation was "broadly encouraging rather than alarming". The Harvey study was criticised by Professor Lars Friberg of the Department of Environmental Hygiene at the Karolinska Institute in Stockholm (*Lancet*, 14 April), who emphasised that the levels found by Harvey's group in liver (up to 28 ppm) corresponded to 200-300 ppm in kidney cortex. These levels are in excess of levels set by the World Health Organisation for the first appearance of adverse effects of cadmium exposure. The new study, conducted by the Department of Clinical Pathology at the Maudsley Hospital, and financed by Thames TV, examined 31 residents of which 22 had abnormal cadmium and seven of whom were excreting the protein β_2 -microglobulin which is taken as a sign of kidney damage. Professor Malcolm Carruthers, Director of Clinical Pathology at the Maudsley says that the problem "needs to be looked at rapidly and with a fair degree of intensity".

India saves the tiger: India's six-year programme to save the tiger from extinction has paid off, according to reports at the First International Symposium on the Tiger held in New Delhi at the end of February. From the 1940s to around 1970, indiscriminate felling of forests, cutting of roads through forests, and the commercialisation of tiger hunting led to a severe depletion of the tiger population: in 1972 only an estimated 1,827 tigers remained in India. The Indian Government introduced a comprehensive Wild Life (Protection) Act in that year, making tiger killing a punishable offence. Project Tiger, funded by the Indian Government, and operated in collaboration with the World Wildlife Fund and the International Union for the Conservation of Nature, was inaugurated in April 1973 and established eleven tiger reserves. The government's commitment is evident in its wholesale removal of villages in the core areas of tiger reserves: in the Kanha reserve alone 4,000 people from 20 villages have been rehoused at a cost of over a million rupees and more reserves are being planned. Another tiger census is being conducted this month, but the tiger population has already increased to 2,484 in its first five years. *From Zaka Imam in New Delhi.*

New WHO report moves to implement barefoot doctor campaign: A specialist committee of the World Health Organisation has proposed a strategy for the development of rural health teams so the goal of "Health for all by the year 2000" can be achieved (8 February). The committee bears in mind that "a permanent improvement in health is unlikely to be achieved without a corresponding improvement in the underlying social and economic conditions" so that a health worker's responsibility is not narrowly technical but also must include promoting healthier living conditions as part of their primary responsibility. WHO considers that the basic components of developmental health care consist of adequate lodging, food and water; suitable waste disposal; provision for pre-natal and post natal infant care including attention to nutrition; immunisation; and accurate health information. The report makes ten general recommendations including one that health workers should not be called "auxiliaries"—and proposes a nine-point national strategy for developing rural health teams.

Training and utilization of auxiliary personnel for rural health teams in developing countries. WHO Technical Report Series, 1978, No. 633 Sf 5.

NIH proposes risk assessment plan for recombinant DNA research: The US National Institutes of Health has announced details of a proposed programme to assess the risks of research involving recombinant DNA techniques, as it had been requested to do so by the Secretary of Health, Education and Welfare, Mr Joseph Califano.

The proposed risk assessment programme, which has been published for public comment, focuses on three types of host-vector systems on which data will be obtained: prokaryotic host-vector systems, primarily *E. coli* and *Bacillus subtilis*; lower eukaryotes, namely *Saccharomyces cerevisiae* and *Neurospora crassa*; and eukaryotic viruses in cultured cells of higher eukaryotes.

To implement the risk assessment programme and co-ordinate research activities, it is proposed that the National Institute of Allergy and Infectious Diseases should appoint an eminent scientist as special assistant to the director. The NIH also proposes that NIAID should set up a Laboratory of Microbial Virulence at Frederick, Maryland, to provide a long-range base for self-initiated risk assessment experiments requiring high containment technology.

Planners backtrack after German nuclear inquiry

Most of the plans for a reprocessing plant at Gorleben are only drafts after all. **Klaus Höpfner** reports on surprise developments at the hearing into West Germany's 'Windscale'

DURING the last big debate on nuclear energy in the West German Bundestag in December 1978, speakers described the present situation over the country's atomic waste as "precarious" and "critical". They placed their hopes in the swift completion of the central nuclear disposal centre at Gorleben in Lower Saxony.

Since then, the storage ponds for burnt-out fuel elements in the German nuclear power stations have filled up still more, the construction of a larger intermediate storage facility at Ahaus in North Rhine-Westphalia has been further delayed, and there are still problems with the temporary dumping of used fuel elements at La Hague in France.

Moreover, plans for the reprocessing and final disposal installations in Gorleben have run into trouble. Severe criticism has been made by 25 international scientists called in by the local *Land* government of Lower Saxony (the authority responsible for plant licensing) to assess the plans. Their 2,200-page report set down little that was encouraging for the pro-nuclear lobby.

During a six-day hearing in Hanover from 28 March to 3 April, these expert critics argued with the Gorleben planners and also explained the plans to a wider public. Initial fears that the press and public would be excluded and denied sensitive information were not borne out. Although the scientists' report has not yet been published, notes of the sessions were distributed and the hearing was open to detailed reporting by the media. Neither has there been any substance so far in the fear that the hearing would merely be a front, and that it would have no influence at all on the decision whether or not to build the plant. On the contrary it seems as if doubts raised about safety and the incompleteness of the existing plans have already begun to have an effect. The Minister-President of Lower Saxony, Ernst Albrecht, made it known, while the hearings were still going on, that the project in its present form would not get his government's approval and would have to be altered. DWK, the German nuclear fuel reprocessing company which is designing and building Gorleben, reacted by declaring that, contrary to all assumptions, large portions of its plans were only preliminary and open to alteration. Only the first phase of construction—the entry storage ponds of

the plant—was decided and needed to be licensed now. Plans for the other phases—reprocessing plant, uranium and plutonium fabrication, vitrification of wastes and final processing—were simply drafts (which nevertheless enabled a decision in principle on technical safety to be made).

The pro-nuclear case at the Hanover hearing was put by representatives from the industrial interests concerned, from nuclear research institutes and other scientific establishments in Germany and elsewhere. On the first day of the hearing, they described the plan as an "optimal draft", for which no alternative existed in the present state of technology. For safety reasons, a fully integrated disposal centre was the best solution. Transport of dangerous loads would be minimised, control of fissionable materials would be made easier, and the costs of guarding and protecting it from attack would be kept relatively small.

Of the three methods for the treatment of used fuel elements which were feasible in principle—namely: reprocessing, long-term intermediate storage with a view to possible reprocessing later on, and final storage in specially built facilities, the decision had been made in favour of reprocessing. This was on ecological grounds (reduction of dangerous materials to a minimum by treatment and re-use as reactor fuel) and on economic grounds (recycling, sparing of scarce uranium resources). Moreover, according to DWK, there was "no technically proven process that would permit highly burnt-out fuel elements to be stored safely over very long periods". Linnemann (Radiation Management Corp., Philadelphia) said it was a "logical, progressive and necessary programme" that would be recognised by many countries and by the International Atomic Energy Agency.

However, the battle over reprocessing must be seen in the context of the commercial use of breeder technology which the German electricity industry is striving for. This is meeting strenuous opposition, as much from the political parties (whose leaders are still leaving the decision open) as from the environmentalists and pressure groups which have achieved considerable success lately at local elections.

Technically, reprocessing is to be by the PUREX process, which, says DWK, "has held good for over 30 years in a large number of treatment plants",

including a small experimental plant in Karlsruhe. Even its advocates admit that the PUREX process, which was developed in the 1940s for military nuclear programmes, still has important technical problems to overcome with the much higher radioactivity of the fuel elements from power reactors (especially in the first stages of the reprocessing, when dealing with the radioactive gases given off). They also concede that many of the steps in the process still have to be adapted for the major technical requirements of a plant with a capacity of 1,500 tonnes a year. Nevertheless, the pro-nuclear supporters in Hanover appeared firmly convinced that all the problems could be solved, and spoke of "normal steps" such as are usual with every advance in technical development.

The general question of the expediency and safety of reprocessing plants, and the technical problem of how far experience with small reprocessing plants can be applied to plants many times bigger became increasingly important during the hearings. The sharpest critics flatly denied that the large-scale reprocessing of reactor fuel elements would ever be feasible. Reprocessing made one problem (the final storage of used fuel elements) into a whole series of problems whose solutions "have never been successfully demonstrated on an industrial scale" according to Walt Patterson (London). It was ecologically senseless because it did not reduce radioactive waste, but increased it, and did so in an uneconomic way, with much higher costs than returns from the nuclear fuels recovered. Scarce uranium stocks could be eked out better in other ways, eg. in enrichment and in the production of the fuel elements. Professor Lindström (Stockholm) tried to quantify the dangers: the problems of reprocessing were "more difficult to the order of magnitude of seven" than those of designing a nuclear reactor, and the risks were correspondingly greater. Mr Schapira (Institut Physique Nucléaire, Orsay), supported this with a long list of only the "most important unsolved problems".

Amory Lovins (London). Professor Barnaby (Stockholm) and others saw the greatest problem in the 14 tonnes of plutonium which would be obtained in the plant each year and would have to be guarded against misuse. The huge capacity of the plant could possibly also be used to treat nuclear fuels for countries which were not a party to the agreement limiting atomic weapons and, in practical terms, to start an export trade in plutonium.



Ghostly garb for anti-nuclear demonstrators in Hanover

Minister-President Albrecht disagreed with the critics on this. The federal government kept strictly to the terms of the non-proliferation treaty. In any case, the treaty ought not to lead to discrimination against countries with no nuclear weapons by denying them access to a closed nuclear fuel cycle—that is, to reprocessing and production of plutonium fuel. From the designers' side, it was added that the plant would not have any extra capacity. That would be to assume that—after clearing up the stocks of atomic waste—there would eventually be 50 nuclear power stations in West Germany. At present there are only 12 (excluding experimental plants) and resistance is growing with every licence that is granted.

Serious misgivings were also raised during the hearings about the safety of the planned entry storage facility, which has been conceived as a "wet store" (water pools in massive concrete structures). A number of scientists warned of the catastrophic consequences of a breakdown in the coolant supply. To this, astonishingly, the designers conceded that the draft, commissioned in 1977, was in fact out of date. They were now thinking of dry storage in metal containers in which the fuel elements would lose heat naturally to the surrounding air (similarly to when they are transported).

Another controversy was about the best way of treating highly active wastes (more than 1,000 cubic metres of concentrated highly active solutions per year), and final storage in the salt domes of Gorleben or in other places with a similar geological formation. Three variants of highly active waste vitrification have been developed in Germany. It is now a question of putting one of them into practice.

For the final storage of radioactive wastes, the *Land* government of Lower Saxony chose the salt dome at Gorleben, near the East German border. But, geologists at Hanover said the geological conditions there were by no means the best. There were sure to be carnallite deposits there and probably anhydrite layers. In storing highly active wastes, special problems would have to be anticipated because of their heat radiation. Building a disposal centre on top of the salt dome would add to the hazard.

Immediately after the six-day hearing, which evoked great public response, particularly in light of the events at Harrisburg, it was clear that there was no basis, either scientifically or politically, for a comprehensive decision in principle.

DWK is now trying to exclude from discussion, for the time being, the most explosive part of the draft—the reprocessing plant. They argue that there is no pressure of time about the reprocessing; that it is not expected before the beginning of the 1990s, and breeder reactors are indeed a matter for the

21st century. There is no hurry either, they say, for the final storage facility, which is scheduled for 1995. Only the further steps for licensing the entry storage facility need to be hurried. Even this, though, should not be too pressing because the major intermediate storage facility at Ahaus should be ready before then.

Meanwhile, there is speculation that Minister-President Albrecht will reduce the decision to one small first step—further investigations into the entry storage facility—and for the rest avoid any commitment on future action. Not only would this gain time to test the feasibility of the plans, but alternatives could be more fully investigated. This applies principally to the option of long-term underground storage with the possibility of retrieval, which has been gaining more and more supporters. In theory at least, this has two advantages: it solves the problem for today and keeps the options open for the future. Its disadvantage is that there has been hardly any experience with it.

Minister-President Albrecht could find himself in a political dilemma in making his decision. He is a member of the executive committee of a political party (the CDU) which is still in favour of nuclear energy. At the same time, Gorleben is anything but popular, and Albrecht governs Lower Saxony with only a bare majority. His opposite number, the former Federal Minister of Building, Ravens (SPD), is only waiting for one wrong move. So Albrecht has now sought an interview with federal Chancellor Schmidt. Without powerful backing from Bonn (where federal Research Minister Hauff has already mounted a big information operation to take the disposal plan to the people), he will be reluctant to give the go-ahead. Since Harrisburg, though, Bonn is even more divided over the nuclear energy question than before. □

Klaus Höpfner is a writer with the Institut für Systemtechnik und Innovationsforschung, Karlsruhe.

'Splash-over' caused Windscale leak

FIGURES on the Windscale leak discovered by borehole sampling (29 March) have finally been released by British Nuclear Fuels. A spokesman said preliminary estimates of the total activity in the leak are from 10,000 to 100,000 curies, the total volume of escaped radioactive liquid is 200 gallons, and the dosage in the borehole is 300 rems per hour. (The International Commission on Radiological Protection calls a dosage of 400 rems lethal.

The leak has been located in an out-

of-use storage site for waste analysis where active liquid from the processing chain was diverted into unused piping to the storage shed due to a "splash-over at a junction point". The maximum activity is 15 to 20 feet under ground and is separated from ground water by a layer of clay. The route of the leak from the storage shed to the borehole is unknown.

BNFL has told police, fire, health service and county council official that there is no hazard to the community or the workforce. □

correspondence

DPAG needs public interest representatives

SIR,—I refer to your article (29 March, page 387) "Britain's shadow science minister believes in experts."

If the substance of your article really does represent the views of Mr Carlisle, perhaps the headline would have more accurately read "believes in non-trade union experts" or even "anti-trade union experts."

The plain fact of the matter is that the Dangerous Pathogens Advisory Group (DPAG), which does not have representatives from the trade unions or from the community, and consists solely of experts in the sense that the Conservative shadow minister uses the word, has in safety terms been a disaster. GMAG, which does include representatives of the public and trade unions, has been able to exercise a positive, constructive and effective role in the area in which it operates. The comparison is dramatic.

Of twelve laboratories cleared by the non-statutory "leave it to the experts" DPAG (which excludes trade union representatives) as safe to handle Category A pathogens, the Health and Safety Executive has now served prohibition/improvement notices on three. No such problems have arisen in laboratories working in the field of genetic manipulation experimentation! These have been controlled from the outset by close cooperation between the Health and Safety Executive and the Genetic Manipulation Advisory Group which does, of course, include the Trades' Union Congress (TUC) nominees and representatives of the community. This is because trade unions' organising staff doing such work can provide experts as good as any, with the added advantage of links with democratic organisations possessing economic and political clout. This makes it possible to defend the interests of the public and employees to a degree DPAG has either been unwilling or unable to do. The consequences have been tragic as we well know from the smallpox occurrence at Birmingham, and the laboratory now closed as unsafe was cleared by a DPAG system Mr Carlisle evidently holds in such high regard.

Would it be fair to conclude that Mr Carlisle is moved by fossilised Conservative policies, rather than the health and safety of the community and of the work people concerned.

Yours faithfully,

R. A. BIRD

Association of Scientific, Technical
and Managerial Staffs,
London.

International energy programmes need coordinating

SIR,—In the face of the approaching threat of a world energy shortage Professor Rotblat's "Call for world

(non-nuclear) energy organisations" (1 March, page 4), to coordinate and stimulate the development of non-nuclear sources of energy seems a positive step. But will this include oil, natural gas and coal? If Professor Rotblat is referring only to the so-called renewable energy sources any means of increasing their contribution beyond the 2% of world energy supply by the year 2000 suggested in a recent report to the Club of Rome deserves encouragement.

Something is, however, already being done: the World Bank Group has lent \$10 billion since 1948 for building power plants. In 1978 alone it lent \$1,146 million for energy and power projects in developing countries—all non-nuclear (almost 20 times the 1978 budget of the IAEA). Furthermore the Centre for Natural Resources, Energy and Transport at the United Nations, as well as the UN Regional Commissions, actively promote all non-nuclear forms of energy, as do UNESCO, UNIDO and UNEP and the US/AID. What is needed is a greater concentration and coordination of the programmes of existing agencies—not a new one.

If the world is to avoid a damaging energy crisis with widespread unemployment, protectionism and even armed conflict over limited energy supplies, all available energy sources—including nuclear power—must be developed, and energy conservation programmes increased. Professor Rotblat is then misguided in attacking what he calls the "promotional" activities of the International Atomic Energy Agency.

In fact a major role of the IAEA is in developing an institutional regime within which nuclear power can be used safely and securely. IAEA's 1979 'assessed' budget provides approximately \$20 million for regulatory activities (of which \$15.6 million are for safeguards against proliferation) compared with \$3.4 million for the division that deals with nuclear power and reactors.

The other promotional activities of the IAEA are in subjects like theoretical physics and fusion or for application of radio-isotopes in biology, medicine, food and agriculture. They are designed to help the great majority of the developing countries for which nuclear power is hardly, if at all, in prospect.

In addition to the "assessed" budget there is a voluntary technical assistance budget. In 1977, 21.9% of this went to nuclear engineering and technology (chiefly training in the safe operation of nuclear plants) compared with about 40% for application of isotopes in agriculture, hydrology, medicine, etc.

Professor Rotblat's main contention that a large expansion of nuclear power would greatly increase the chance of nuclear war, since almost every nation would have access to plutonium, flies in the face of the facts of history, economics and technological development. All nuclear weapon programmes have been based on series of special facilities dedicated solely to

military purposes and quite independent of the civil power programmes of the countries concerned. This separation becomes even more marked as time goes on. It is far cheaper, easier and less conspicuous to build and operate the small purpose-built plants needed for a modest weapon programme than to engage in commercial nuclear power. The difference in cost is of the order of twenty to one. Stopping nuclear power would not cause a moment's pause in the nuclear armament and submarine programmes of the weapon states.

A more immediate threat to world peace comes from competition over limited supplies of oil. According to a report in the *Financial Times*, 26 February, 1979, the US Defence Secretary has said that in protection of vital Middle East oil supplies "we will take any action that is appropriate, including military force". By increasing energy supplies and reducing the pressure of demand over scarce resources nuclear power will prove to be a stabilising factor in international affairs.

Professor Rotblat may not think much of treaties and international safeguards, but the fact is that the rate of proliferation of nuclear weapons has declined each decade since 1944. At the same time almost all the industrial and many of the leading developing countries of the world have benefitted from the peaceful use of nuclear technology. The danger of nuclear war comes from the arsenals of the great powers and not the power plants of the small.

Yours faithfully,

G. GREENHALGH

London, SW15.

Recombinant DNA dangers and wild *E. coli*

SIR,—Many scientists engaged in recombinant DNA research now expound the doctrine, referred to by Eleanor Lawrence in her report on the COGENE meeting at Wye (12 April, page 590), that the dangers that might arise during the course of their experiments cannot be greater than the dangers posed by working with the most dangerous organisms employed in those experiments. This might well be so when the recipients of genetic material are 'enfeebled' organisms such as *E. coli* K12, but it could be far from true if other recipients were employed, deliberately or accidentally. For example, to my mind, the implantation of efficiently expressible genes for botulinum toxin production in wild *E. coli* or other organisms that can multiply relatively freely in the food or in the intestinal tract of man or animals represents a much greater danger than the presence of these genes in *Clostridium botulinum* itself.

Yours faithfully,

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news and views

A pioneer's view of Venus

from Garry E. Hunt

VENUS, our nearest planetary neighbour, has long been thought to be the Earth's sister, because of its similarity in size and mass. But the rapidly advancing knowledge of Venus gained from Earth-based telescope studies, and the measurements taken during space missions, has shown that the planets are actually quite different. Without doubt, the most dramatic development in our understanding of Venus, and one that has also changed our theories of how the Solar System was formed, has come from the recent Pioneer multiprobe orbiter mission, whose initial results were described recently in *Science* (203, 743; 1979). (Except where stated all following references are to that issue).

Descending through the thick atmosphere, the neutral mass spectrometer (Hoffman *et al.*) and the gas chromatograph (Oyama *et al.*) showed surprisingly large concentrations of ^{36}Ar , ^{38}Ar and ^{20}Ne . The volume mixing ratio of ^{36}Ar in the Venusian atmosphere is approximately 90 times greater than that of the Earth's. It follows that the absolute abundance of ^{36}Ar in the Venusian atmosphere must be approximately 200 to 300 times larger than the Earth's. The abundance of ^{20}Ne is also higher in the Venusian atmosphere and comparable to that of ^{36}Ar . The ratio $^{36}\text{Ar}/^{38}\text{Ar}$ is similar to values observed for the Earth, meteorites and the Moon.

The ^{36}Ar measurements have a profound effect on theories of the formation of the Solar System. Taking the Viking measurements for Mars into account (see for example Anders & Owen *Science* 198, 453; 1977) we now find that the amount of argon decreases from Venus to Mars. This suggests that the solar nebula, out of which the planets were formed, had an inhomogeneous distribution of material. The inert gases, such as argon, were trapped in the grains which formed the planets and settled as a veneer on these bodies as they condensed out of the nebula. These discoveries by Pioneer Venus have clearly reshaped our thinking on this important

subject. To obtain a more detailed picture we need similar measurements of other Solar System objects, and in the foreseeable future the probe measurements of Jupiter and the studies of cometary atmospheres could yield invaluable data.

The multiprobe system on the Pioneer spacecraft consisted of a bus transporting a large probe and three small probes designed to land at a northern latitude, on the day side and night side of the planet. In this way a global picture may be developed from this set of measurements. The contrast of the atmospheric structure below an altitude of 50 km at the widely differing locations was found by Seiff *et al.* to be no more than a few degrees Kelvin, with slightly warmer temperatures at 30°S than at 5° or 60°N . The atmosphere was stably stratified above 15–20 km, suggesting that near adiabatic lapse rate is maintained by the atmospheric motions. The profiles move from near adiabatic toward radiative equilibrium, at altitudes above 40 km. There seems to be a region of vertical convection above the dense cloud deck, in the altitude range of 47.9 to 49 km, where the temperature is approximately 360 K.

Venus is covered with a ubiquitous layer(s) of cloud, whose structure is intimately associated with the observed temperature structures. From their orbiter measurements, Travis *et al.* have detected a substantial layer of sub-micron particles of thickness 0.05–0.1 in the lower stratosphere, well above the main sulphuric acid haze layer. Stewart *et al.* find evidence of albedo features at 2068 Å which corresponds to the bright and dark features seen in the Mariner 10 images. The dark regions may therefore be locations where SO_2 is strongly absorbing. Taylor *et al.* find temperature contrasts of 50 K between a cold circumpolar band. Beneath these visible clouds, the layers of material have now been examined in detail by the Pioneer instruments.

A middle cloud region exists between 51 and 56 km and shows a decrease in concentration to about 100 cm^{-3} according to Knollenberg and Hunter. This layer appears to contain both liquid particles and solid grains of

radius 10–20 μm . The dense layer lies between 49 and 52 km with the visibility restricted to less than 1 km. This is the region of largest particle sizes observed. Just beneath this thick layer is a distinct thin layer of somewhat reduced opacity.

The region from 32 to 48 km below these clouds possesses a thin haze of particles nearly $1\text{ }\mu\text{m}$ in size with concentrations of 1 to 20 cm^{-3} . The upper part from 47–49 km is most dense and corresponds to the region of convective activity observed by Seiff *et al.* Below the lower boundary the atmosphere appears to be free of particles all the way to the surface. However, this does not mean that the visibility is unlimited. The Venusian atmosphere is so dense that the scattering of light by gas molecules alone will hide the surface from view.

The general behaviour of the measurements reported by Knollenberg and Hunter, and Ragert and Blamont indicate a concentration of particles independent of height or decreasing at lower altitudes. This behaviour is consistent with a source at the top and a sink at the bottom, and a downward flow by mixing and by gravitational settling. This suggests there is a source of sulphur and H_2SO_4 at or about 57 km, which is the top of the middle cloud region. The source is presumably photochemical in nature, requiring ultraviolet radiation that does not penetrate far into the sulphur cloud. The H_2SO_4 droplets lose water on the way down, but they encounter increased H_2O and perhaps SO_2 and SO_3 to form the lower cloud region of dilute acid and the densest layer. Most important, the large particle sizes ($>10\text{ }\mu\text{m}$) encourage the possibility of precipitation on Venus either in the dilute H_2SO_4 lower cloud region, or initiated by sulphur from the middle cloud region, which would pick up dilute H_2SO_4 when falling below 51 km.

Several substances, besides water, are almost nonexistent, above the clouds, but well represented below them. The abundance of SO_2 varies in this way by a factor of 240 and oxygen by 60 (Oyama *et al.*). These higher abundances arise when sulphuric acid and sulphur rain out of the clouds, vapour-

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ise and even dissociate in the heat below and then chemically attack gases found in the lower atmosphere.

The presence of SO_2 and not COS (Prinn *Science* **182**, 1132; 1977) as the major sulphur-bearing compound in the lower atmosphere, is a great surprise, (Oyama *et al.*). Previously it was thought that the abundance of COS indicated that the amount of CO in the atmosphere is buffered by certain minerals at the surface, and its value is representative of the amount below the clouds (Lewis *Earth. Planet. Sci. Lett.* **10**, 73; 1973). Now these new measurements suggest that the CO (approximately 50 p.p.m.) is generated almost entirely by photochemical processes and that no significant amounts of COS should be expected.

The Pioneer measurements confirmed the surface temperature at $\sim 737\text{ K}$ and reasonably uniform, at the base of this huge CO_2 atmosphere. Since Venus and the Earth are nearly planetary twins, why are their atmospheres so different? This is not simply an academic question. The planets would have had some basic similarities in their initial state and the processes that have transformed Venus into its current inhospitable state might still occur on Earth.

Venus is much nearer the Sun and receives twice the incident sunlight that reaches the Earth. The additional heat would have quickly produced an efficient 'greenhouse' effect, increasing the atmospheric CO_2 opacity and raising the surface temperature to a level which would be too hot for any water vapour to condense into oceans. It is therefore important that we fully understand the evolutionary process of Venus to gain a better perspective of the possible perturbations that may affect the Earth's atmosphere.

Now, Tomosko *et al.* have found that only 2% of the incident sunlight reaches the surface. However, for the greenhouse effect to be maintained, Bolse *et al.* find that the contribution from the water vapour, whose mixing ratio is only 10^{-3} , is essential to provide the additional opacity which combines with the contributions from CO_2 and the cloud particles.

What is the origin of the present-day Venusian atmosphere? Observations of the surface of the planet are essential to address this problem, but this is obviously difficult because of the extensive cloud layers. Radar maps of the surface are being produced from measurements made from the orbiter (Pettengill *et al.*). Although the measurements are restricted to the region bounded by 74°N and 63°S , the high resolution observations show evidence of a rift valley having vertical relief of up to 7 km. There is almost the suggestion of volcanic features, which may indeed be responsible for the creation of the

current atmosphere. The surface of Venus may therefore have some similarities with other terrestrial planets.

One of the most startling observations was made as the spacecraft descended toward the surface, where in the altitude range of 6–14 km, lightning storms were encountered. At least 1,000 impulses of radio noise were measured. Lightning on Venus is not unreasonable, since the atmosphere is electrically active. By way of comparison, there are typically 100 lightning strokes every second scattered all over the Earth. But on Venus there may be several times that number in a localised area. Instead of being illuminated for a brief instant by a dazzling flash of lightning, as on Earth, the Venusians may well be glowing from the nearly continuous discharges of those frequent lightning strokes. We have also gained a bonus measurement from the day probe which unexpectedly survived on the surface for more than an hour. It impacted on a surface composed of loosely compacted material so that a small amount of fine dust was ejected from the surface into the atmosphere in the vicinity of the probe (Ragen & Blamont). The dust subsequently settled out on to the surface in a few minutes.

The orbiting spacecraft travels on an eccentric path with a periapsis altitude of $\sim 200\text{ km}$ and apoapsis of $\sim 67,000\text{ km}$, and carries a range of instruments to investigate the properties of the upper atmosphere. Taylor *et al.* find that in the 60–140 km region of the atmosphere, there are small diurnal temperature differences near the cloud tops which increase with altitude. The pole-to-equator tempera-

ture contrasts are much larger. An isothermal layer of at least two scale heights in the vertical is found near the 100-kilometre altitude, where the temperature is $\sim 175\text{ K}$. The monitoring of this region above and in the neighbourhood of the cloud top is important in determining the mechanisms of the meteorology of this region.

In spite of the absence of any appreciable magnetic field, the ionosphere of Venus seems to repel the solar wind at least as well as the terrestrial magnetosphere (Wolfe *et al.*; Scarf *et al.*). At Venus, streams of particles and radiation from the Sun confine the ionosphere below a well defined boundary. This is called the ionopause, and is not found around the Earth. One of the major surprises is the presence of the ionosphere on the night side of Venus. The ions and electrons that produce this region should recombine when the Sun is removed as an excitation source. On Venus, the night lasts for 8 weeks, so it was at first thought that the ionosphere should disappear (Taylor *et al.*). Perhaps the atmosphere at these levels rotates more rapidly than the planet and therefore prevents the charged particles from neutralising.

The Pioneer Venus mission, with the orbiter and sophisticated probe systems has provided a great deal of information on our nearest neighbour. The *in situ* measurements of the atmosphere have produced surprising information that has changed our understanding of the formation of the Solar System. This is an amazing accomplishment. Detailed exploration of the planets always yields surprises and gives a new perspective on the Earth. $\square$

Nerves, molecules and embryos

by Miranda Robertson

THE interactions of nerve cells with one another and with their environment at any stage are incompletely understood; the least understood of all are those that take place during embryonic development. Two philosophies can be discerned among biologists in this difficult field. One is that developing animals are so complicated that experiments on them cannot possibly yield interpretable information on the molecular mechanisms underlying development, and the only reasonable strategy is to investigate developing cells *in vitro*. The other is that because development is so complex

and the topology of the developing animal so important, nothing interesting can be learned about its mechanisms from cells, or even tissues, in culture.

They are probably both right; and a recent meeting* largely devoted to neural interactions in development therefore started propitiously with a talk from N.M. Le Douarin (CNRS Nogent-sur-Marne) whose work on neurochemical differentiation in chicks has recently run in reassuring parallel with that of the Harvard group, represented at the meeting by L. Chun and D. Potter, on neurochemical differentiation in Petri dishes.

The issue is what determines whether a sympathetic ganglion cell will become noradrenergic or cholinergic. Le

*A meeting on Cell-Cell Interactions in Neurobiology was held on 12–15 February, in San Diego. It was partly sponsored by the National Foundation March of Dimes.

Douarin's contributions to this question are based on the use of tissue grafts from quail embryos into chick: the grafted cells participate in the embryonic development of the chick, where their progeny can be identified by a stable nucleolar marker peculiar to quail. By grafting small pieces of quail neural crest into more anterior or more posterior positions in chick neural crest, she has been able to show that the migration pattern of the cells to the sympathetic ganglia and other structures is determined by their position on the antero-posterior axis, their neurochemical differentiation probably being induced by tissues at their destination.

Investigations by the Harvard group have suggested that the inducing influence is a soluble factor released by the target organs of the neurones: adrenergic neurones dissociated from the superior cervical ganglion of the rat are induced to express cholinergic functions instead if they are cultured with (for example) heart cells, or medium in which heart cells have been growing ('conditioned medium'). The existence of soluble factors would imply that direct cell contact is not necessary, but there are indications from preliminary experiments by E. Hawrot on ganglion cells cocultured with dead heart tissue that a cell surface factor may also exist (L. Chun, Harvard University Medical School).

The discovery that ganglion cell neurochemistry can be manipulated in this way has raised the question of how stable the differentiation of neural crest cells is even after they have reached their destinations. This stimulated Le Douarin to show that a similar switch can be induced *in vivo* by transplanting cholinergic ganglion cells from 4-6-day quail embryos to a potentially adrenergic region in the neural crest of chick embryos at an earlier stage of development (Le Douarin *et al. Proc. natn. Acad. Sci. U.S.A.* **75**, 2030; 1978). It has yet to be established how late in development ganglion cell differentiation remains reversible, but there is now some evidence that activity induced by presynaptic fibres may be what stabilises transmitter choice. This was originally suggested by some experiments of Black *et al. (Brain Res.* **34**, 229; 1971), and the Harvard group has been able to show that depolarisation induced by various means can prevent the effects of conditioned medium on cultured ganglion cells (Walicke *et al. Proc. natn. Acad. Sci. U.S.A.* **74**, 5767; 1977). The importance of the soluble factor in conditioned medium is that it specifically induces differentiation, without affecting growth or survival (by contrast with nerve growth factor, for example, which enhances growth and survival without influenc-

ing differentiation). It is therefore crucial to establish clearly that conditioned medium truly causes phenotypic switching and not merely the differential survival of cholinergic cells in an already heterogeneous population. The Harvard group now has persuasive evidence for switching, from the results of experiments in which one or at most two cervical ganglion neurones are grown on a bed of heart tissue with which they can synapse. By monitoring the effects on heart cell contraction of stimulating the individual neurones, they have been able to show that neurones which by all available criteria were adrenergic at the start, become dual function (presumably in the process of switching) or cholinergic in such culture conditions.

Soluble factors

One consequence of Petri-dish embryology is that developmental neurobiology is beginning seriously to rival immunology in the proliferation of uncharacterised or semi-characterised 'factors' which are identified by their important but ill-understood actions on cells *in vitro*. G. Fischbach (Harvard Medical School) has made a start on the characterisation of a factor from spinal cord explants which will induce within hours an increase in both the density and the clustering of acetylcholine receptors on cultured chick myotube. From its molecular weight and sensitivity to tryptic digestion, the factor seems to be a small peptide, possibly related to the activity reported by Podleski *et al. (Proc. natn. Acad. Sci. U.S.A.* **75**, 2035; 1978) in nerve extracts and which has similar effects on cultured rat muscle. Its relationship to those described by Anderson *et al. (J. Physiol. Lond.* **268**, 731 & 757; 1977) and Christian *et al. (Proc. natn. Acad. Sci. U.S.A.* **75**, 4011; 1978) from neural tube and neuroblastoma/glioma hybrids respectively is more problematical, since in both these cases there was receptor clustering without any significant increase in receptor density. The importance of factors influencing receptor distribution is that they may play a part in neuromuscular synaptogenesis. A recent controversy about whether the receptor clusters that form at the site of the synapse are intrinsic to the muscle or induced by the nerve has recently been settled in favour of the nerve (see *News & Views* **278**, 599; 1979); but the mechanism remains unknown.

Another factor was discussed by J. Brockes (California Institute of Technology), whose long-term aim is to find out what determines the interactions of

Schwann cells with the neurones they myelinate. Schwann cells can be induced to proliferate in culture either by agents which elevate intracellular cyclic AMP, or by a factor which is present in extracts of bovine pituitary and brain but which does not seem to be any of the more obvious hormones or growth factors, which have no effect on Schwann cell division. The factor is not found in non-neural tissue and is a basic protein of molecular weight about 50,000. While the signal that induces proliferation is thus partly characterised, the signal for myelination is so far detectable only operationally. Schwann cells distinguish between small-diameter axons, which they enfold without myelinating them, and larger ones around which they wrap their membrane to generate the myelin sheath. Brockes reported that Schwann cells make detectable amounts of the major peripheral myelin proteins only when stimulated to do so by contact with appropriate axons, and interact with artificial fibres in the way that they do with unmyelinated axons. This suggests that Schwann cells are induced to myelinate by a chemical signal from certain axons, rather than simply by the geometry of large-diameter fibres. Considerably more elusive than factors influencing differentiation have been those that influence morphogenesis; but it is increasingly believed that differential stickiness of cell surfaces may account for much specific contact-making, both between individual cells and between sheets of tissue. Dennis Bray (Washington University Medical School, St. Louis), investigating the general question of the mechanism of neurite outgrowth, presented results which may suggest how differential adhesion could direct the growth of nerve fibres. Growing neurites end in a specialised growth cone, from which filopodia extend symmetrically to attach to the substrate. By pulling the neurite gently to one side or the other, Bray has shown that the direction of growth follows the main axis of tension.

The importance of stickiness

So far, there has been no attempt to test theories of differential adhesiveness on actual embryos; but it became clear at the meeting that this could and should be done, specifically in the case of the adhesive molecule that G. M. Edelman and U. Rutishauser (Rockefeller University) have identified in cultures of chick retinal and tectal cells. It now seems possible that adhesive molecules on the surface of retinal cells may be the basis for the strict ordering of their connections with the optic tectum which it is now believed may be determined simply by the preservation of neighbour relations between

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growing axons. This notion has recently been checked anatomically by J. Scholes (*Nature*, **278**, 620; 1979) against the optic nerve of Cichlid fish, which is ideal for such studies because it takes the form of a ribbon in which the radial coordinates of the retina can be shown by retrograde degeneration to map across its width, and the circumferential coordinates across its thickness. (Much the easiest way to envisage this geometry is to see the retina as a fully opened circular fan: close the fan and you have the beginning of a ribbon.) Scholes has established that neighbour relations are preserved with very little deviation for the whole length of the nerve almost up to the tectum, when discrete bundles of fibres (within which neighbour relations continue to be preserved) undergo a complex rearrangement before spreading out on the tectal surface.

Stickiness, clearly, could explain this kind of ordering, particularly since the retina grows in successive rings from centre to periphery, and new axons could thus follow the adhesive surface of the neighbouring older axon. The work of the Rockefeller group on a cell adhesion molecule (CAM) in cultures of chick retinal and tectal cells (see *J. Cell Biol.* **79**, 371, 382; 1978) may provide a biochemical test for the nearest-neighbour hypothesis. They have used antibodies to CAM to disrupt such ordering as occurs in cultures of retinal cell aggregates and spinal ganglia in a way that suggests that CAM mediates the sorting out of cells and reunions to form tissue layers, and the antibody can inhibit the side-to-side adhesion between neurites that produces nerve bundles. It may thus be possible directly to test the nearest neighbour hypothesis by injecting anti-CAM into embryos during optic nerve development and looking to see if the retino-tectal map is scrambled.

While there is now some hope of discovering the molecular basis for short-range effects such as the induction of differentiation and the guidance of growing axons, the long-range or global control of morphogenesis remains as mysterious as ever. What, for example, causes the optic nerve fibres to undergo a systematic piecewise rearrangement at the tectum (Scholes *op. cit.*)? Indeed, what in general determines the orientation of developing systems? This question was raised in J. Cooke's (National Institute for Medical Research, London) discussion of his recent work, which is concerned with the determination of orientation in the tadpole eye. If a tadpole's eye is rotated at any stage after the first few hours after spawning, the growing nerve fibres will turn before they reach the tectum and make the connections they would have made from a normal

eye, thus giving the frog a rotated visual map. According to some classical experiments by M. Jacobson (*Science* **155**, 1106; 1967; *Devl Biol.* **17**, 202; 1968), eyes rotated at earlier stages become respecified, making connections appropriate to the rotated eye and producing a normal visual map in the adult frog. Cooke has failed to repeat this finding and in the course of seeking the reasons for this failure he has touched on the kind of developmental phenomenon that is most resistant of all to molecular explanation.

Cooke and his colleagues never find normal maps from rotated tadpole eyes, no matter how early the rotation: they find either rotated maps, or compound maps in which half the visual field is rotated and half normal. By using albino tadpoles as the hosts and grafting rotated eyes from wild-type tadpoles, they have been able to establish from the histology that the compound maps are due to the partial regrowth of the host eye. They suspect that Jacobson's results may therefore have been due to the regeneration of normally oriented eye tissue, the absence of compound maps being due either to the death of the rotated tissue or to a gradual takeover by the regenerating tissue in the course of growth—since many of Jacobson's animals were mapped when relatively mature.

These experiments led Cooke to the question of whether in fact it is the rotation itself that induces the regeneration of host eye tissue. This question arises from the attempts of French *et al.* (*Science* **193**, 969; 1976) to explain the occasional duplication of regenerating structures. Their proposal which has become known as the 'clockface' model, is that growing structures are specified according to circular coordinates and that rotated structures can give rise to duplications at the site of the graft because of intercalary regeneration between discrepant clock-face values. To test whether rotation is thus causing the regenerative duplication of the retina in tadpoles, Cooke has experimented with grafts of unrotated eyes; and preliminary results suggest that in these animals regeneration does not occur.

This kind of interaction is particularly difficult to explain by any imaginable molecular mechanism. Not that any actual molecular mechanism has yet been found to account for the relatively approachable problem of Cartesian positional gradients, which are generally believed to account for most morphogenetic signalling. Scholes has pointed out that the retinal circular fan is a way of transforming circular to Cartesian coordinates. This may not help with the identification of a morphogenetic model; but it could point the way to a unified theory. □

Is DNA really a double helix?

from Struther Arnott

ALTHOUGH it is 26 years since the announcement of the base-paired double-helical structure for DNA, there are still attempts to answer questions about DNA without using the Watson-Crick paradigm. For example, Rodley *et al.* (*Proc. natn. Acad. Sci. U.S.A.* **73**, 2959; 1976), Cyrax and G  th (*Naturwissenschaften* **65**, 106; 1978), and Sasisekharan *et al.* (*Proc. natn. Acad. Sci. U.S.A.* **75**, 4092; 1978) have all proposed that the two strands of DNA do not coil plectonemically round one another. Thus, the problem of unwinding extensive lengths of cohelical DNA for replication is abolished at a stroke and new roles will have to be postulated for relaxase, gyrase, helicase and so on.

Rodley's structure is typical. It retains the antiparallel, complementary base-paired chains of the Watson-Crick structure and the 10-base pair axial repeat of B-DNA. Lack of mutual coiling is contrived by irregular chain conformations in which blocks of about five nucleotides have right- and left-handed twistings alternately. The resulting model is analogous to a warped zipper. Since the phosphate groups lie more or less on the surface of a cylinder with radius about 9    and the base-pairs are stacked 3.4    apart along the long axis of the molecule, it has many features in common with the double-helical model of B-DNA. The calculated diffraction pattern is therefore encouragingly similar to that observed from DNA fibres. Had such a model been available in 1953, it would have been regarded as a serious competitor for the original bihelical model.

Since 1953, the original DNA model has been substantially modified by Wilkin's school and refined to fit the >100 X-ray diffraction intensities which can be obtained from oriented and polycrystalline fibres of B-DNA. Recently, R. Chandrasekaran and I remeasured these intensities and re-refined the double-helical model of B-DNA which now provides a reliability index $R=0.28$, which is reasonable when one considers that fixed standard bond lengths and angles have been imposed on the model as well as specific base and furanose ring shapes leaving the six conformation angles of the generating nucleotide as the only variables. Poorer models have higher values for R . For example, a double-helical

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model with an unacceptable alternative base-pairing scheme has $R=0.45$. For the Rodley model $R=0.48$. These less regular warped zipper models have 10 times the degrees of freedom allowed to regular helices and would have to have $R<0.20$ to be regarded as being significantly superior to the best bihelical model according to Hamilton's test (*Acta Cryst.* **18**, 502; 1965).

It is not unthinkable to me that a warped zipper model could be contrived with R rather less than the present value of 0.48. The effort required would be considerable and, in the circumstances, one is inclined to look for more decisive experiments not requiring accurate knowledge of the atomic positions in DNA. Crick *et al.* (*J. molec. Biol.* **129**, 449; 1979) believe that studies of closed circular DNA molecules will suffice. When a closed circular DNA with N base pairs is unwound by known amounts, one can calculate the net number of times each chain is coiled round the other in relaxed DNA. This linkage number (α) turns out to be approximately $N/10$ as would be expected from the classical bihelical model rather than near zero as predicted for the warped zipper models. (By retaining a modest amount of coiling as a result of failing to balance exactly the right- and left-handed twistings, the proponents of the warped zipper structures have contrived that $\alpha \neq 0$ and avoided having to explain why closed circular DNA duplexes cannot be melted to give two separable classes of single-stranded circles.) Crick *et al.* argue also that the quantised migration of supercoiled DNA molecules in gel electrophoresis can arise only from the fact that all the molecules in one band have the same linkage number which differs by unity from that of molecules in adjacent bands, and that this can be explained only by assuming that the molecular secondary structure is double helical.

There is also crystallographic evidence that nucleic acid duplexes are regular helices rather than asymmetric zippers which does not require accurate knowledge of atomic coordinates. Intuitively, one might expect that molecules with fourfold screw symmetry (like the eightfold helices of *D*-DNA) would crystallise with tetragonal symmetry. They do. Similarly, one is not surprised to find that the 12-fold helices of *A'*-RNA crystallise with rhombohedral symmetry. The crystalline arrangements which would permit 11- or 10-fold helices to exploit the equivalence of many residues and maximise the number of favourable contacts between molecules are less intuitively obvious and might be expected to be rather peculiar. On the other hand, if such peculiar packing arrangements are indeed observed in

polynucleotide crystals, it would be difficult to escape the conclusion that the crystals contained 10- or 11-fold helices. *A*-RNA duplexes which have been described as 11-fold helices by me and my colleagues (*Nature* **211**, 227; 1966) provide a particularly compelling example. *A*-RNA duplexes pack in trigonal lattices containing two sets of close-packed triads. Although the triads are the same size, the contacts within them need not be the same. Indeed, for zipper molecules with no helical symmetry, they could never be the same. For 11-fold helical molecules, the contacts within both triads can be the same provided that the value of a particular packing parameter x is 0.368 or 0.458 (Arnott *et al.* *J. molec. Biol.* **27**, 549; 1967). It is a striking fact that there are two crystal forms of *A*-RNA, one with $x=0.365 \pm 0.005$ and one with $x=0.460 \pm 0.005$. Of course our credulity could be stretched by the argument that helical models should be aban-

doned for *B*-DNA alone among the dozen or so polynucleotide duplexes studied by X-ray diffraction. Fortunately, *B*-DNA crystals exhibit the analogous properties which would be expected of 10-fold helices. The unit cell is rectangular and contains two molecules. If these molecules are 10-fold helices and the number of equivalent contacts is maximised, the ratio of the unit cell sides a and b should be $\tan 2\pi/10$. The remarkable fact is that b/a is $\tan(36.15^\circ \pm 0.25^\circ)$. I find it hard to believe that these crystallographic expectations would be realised so accurately if polynucleotide duplexes in general and *B*-DNA in particular were not close to being perfectly regular helices.

For many years attention was concentrated on the base pairing of DNA. It was right that the backbone should be subjected to similar scrutiny. It is unlikely that any major modification will be needed. $\square$

Improved laser test of the isotropy of space

from Bridget Marx

It might be thought that the most sensitive tests of the special theory of relativity would be those performed on systems travelling close to the speed of light. However, a recent experiment by Brilliet and Hall (*Phys. Rev. Lett.* **42**, 549; 1979) which relies on a velocity of only 10^{-4} times that of light has produced one of the most precise tests of the Lorentz transformation, and hence special relativity, yet achieved. The basic ideas of the experiment date from 1887 when Michelson and Morley performed their now classic 'ether drift' measurement. That experiment has been improved, modified and repeated several times in the intervening years with little more than an order of magnitude improvement in sensitivity. The new measurement extends the precision of the previous best null result by a factor of 4,000 in the fractional frequency shift.

The basis of Michelson-Morley type experiments is to look for an anisotropy in the velocity of light travelling in a moving frame of reference. The anisotropy for which Michelson and Morley (*Am. J. Sci.* **34**, 333; 1887) were looking was to be attributed to the effect of the Earth moving through the ether, in which it was supposed that light propagated. They measured the round trip times for light to travel in two per-

pendicular directions by looking at the resultant interference produced between the two arms of an interferometer, and then rotated the whole apparatus. They did not see the expected fringe shift and their resolution placed an upper velocity of 5 km s^{-1} on the Earth's motion through absolute space, in which the ether was thought to be at rest. An improved experiment by Joos in 1930 (*Ann. Phys.* **7**, 385; 1930) reduced the limit to 1.5 km s^{-1} . This was far below the expected value of about 30 km s^{-1} , the Earth's velocity about the Sun.

The null result of the Michelson-Morley experiment is considered very

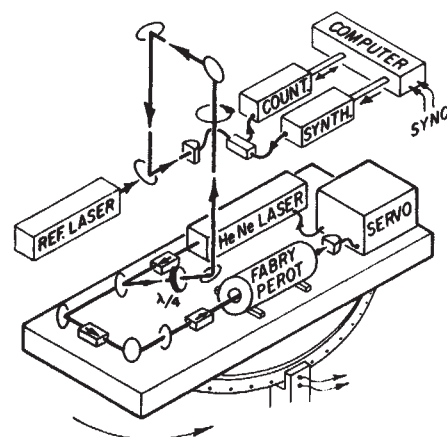


Fig. 1 Schematic of isotropy-of-space experiment. (From Brilliet & Hall *op. cit.*).

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important to the foundation of Einstein's theory of special relativity. Indeed Robertson (*Rev. Mod. Phys.* **21**, 378; 1949), in an attempt to replace postulate with observation in special relativity, has derived the Lorentz transformation by assuming the existence of a Euclidean rest-frame in which light propagates isotropically and using the idealised results of the experiments of Michelson and Morley, Kennedy and Thorndike (*Phys. Rev.* **42**, 400; 1932) and Ives and Stilwell (*JOSA* **28**, 215; 1938). In Robertson's rest-frame there is a 'metric', with which one can measure space or time intervals, defined as

$$d\sigma^2 = dt^2 - c^{-2}(dx^2 + dy^2 + dz^2)$$

c being the velocity of light in this frame. In an inertial frame moving with velocity v along x the metric is transformed to

$$d\sigma^2 = g_{00}(v)dt^2 - c^{-2}(g_{11}(v)dx^2 + g_{22}(v)(dy^2 + dz^2))$$

If $g_{00} = g_{11} = g_{22} = 1$ then this metric transform is exactly equivalent to the Lorentz transformation. If $g_{11}(v) = g_{22}(v)$ then space in the moving frame is isotropic and the Lorentz contraction follows. The three experiments mentioned above measure g_{11}/g_{00} , g_{00}/g_{11} and g_{00} respectively. In fact, the fractional frequency shift observed in the Michelson-Morley type experiments determines $1/2((g_{22}/g_{11}) - 1)$, which would approximately equal $\frac{1}{2}v^2/c^2$ in the ether theory. The previous best limit to the value $g_{22}/g_{11} - 1$ was that obtained by Jaseja *et al.* (*Phys. Rev. A* **133**, 1221; 1964) and was $\pm 2 \times 10^{-11}$. This was obtained by measuring the beat frequency between two rotating lasers, the cavities of which were perpendicular to each other. Unfortunately, large spurious noise signals limited their sensitivity to only a factor of three better than Joos had obtained.

The new experiment of Brillet and Hall gives a limit on $(g_{22}/g_{11} - 1)$ of $\pm 5 \times 10^{-15}$, which is a significant improvement over the previous results. Their experimental arrangement is shown schematically in Fig. 1. As the length standard they use a single Fabry-Perot etalon, which is mounted on a table rotating in a horizontal plane. Also on the table is a helium-neon laser at around $3.39 \mu\text{m}$ whose output frequency is locked by a servocontrol system to the length of the etalon. Any change in the round trip time of the laser light in the etalon will result in a compensatory frequency shift of the laser, so that the laser wavelength always satisfies a standing-wave condition in the etalon. The etalon is very stable, made of low expansion glass and temperature controlled to within 0.2°C .

Using this kind of servo-system the laser frequency on a non-rotating table is observed to remain constant to within 20 Hz in a 1 s sampling time. The frequency of this cavity-locked laser is compared with that of a stationary helium-neon laser which is locked to an absorption line in methane, and has a similar frequency stability. Isolation of all parts of the system is important, and to this end the methane-stabilised laser is used to lock a third helium-neon laser at an offset of 120 MHz and this is then used to produce a beat frequency with the rotating, cavity-stabilised laser. Faraday isolators prevent feedback from the Fabry-Perot cavity to its laser. The excellent results achieved in this experiment are attributable partly to the attention paid to stability in all parts of the apparatus and partly to the sophisticated signal averaging techniques applied to the beat frequency data. Any spatial anisotropy will manifest itself in a beat frequency change, due to a change in the frequency of the rotating laser, periodic at twice the table rotation frequency. Each reading, which takes approximately half an hour, is the average of many table revolutions and the time dependence of the beat frequency is analysed to select the changes with the right periodicity. The resulting fractional frequency change is of the order of 2×10^{-13} . However, by taking readings at different times of the day and over 100 days this was reduced. A

genuine anisotropy signal will vary in phase according to our velocity direction through space. Spurious signals will remain largely with a fixed phase, and can thus be partly eliminated by looking for a 24-h phase rotation. By also deciding on what is the fixed, isotropic frame with respect to which the Earth is moving, it is possible also to take into account a yearly phase rotation and thus further reduce the spurious background. This averaging is assumed valid as there is no noticeable hour or day dependence of the data. This brings the observed fractional frequency shift down to 10^{-14} for a 24 h average and $1.3 \pm 2.2 \times 10^{-15}$ for the overall average. This result limits $(g_{22}/g_{11} - 1)$, the spatial anisotropy factor in Robertson's derivation, to $3 \pm 5 \times 10^{-15}$. In terms of the ether theory this corresponds to a limiting velocity of about 20 m s^{-1} .

If we assume that the isotropic rest-frame with which we are comparing our moving frame is that in which the cosmic blackbody radiation is isotropic, then the velocity of the Earth is approximately 400 km s^{-1} . Using this velocity the null result of Brillet and Hall, with a resolution (sensitivity divided by predicted ether shift) of about 3×10^{-8} , becomes the most sensitive test of the Lorentz transformation yet performed. If the sensitivity is improved further it might soon be possible to observe the breakdown of this transformation as the anisotropies introduced by general relativistic effects become important.

Eels: new interest in an old problem

from J. D. McCleave and F. R. Harden-Jones

A RECONNAISSANCE expedition to the Sargasso Sea now underway may help to clear up a long-standing mystery. Where are the spawning grounds of the North Atlantic eels?

The American and European eels (*Anguilla rostrata* and *A. anguilla*) of the North Atlantic are very similar in appearance but individuals can be separated by their mean vertebral counts of 107 s.d. ± 1 and 115 s.d. ± 1 respectively. The eels are migratory. According to the classical account of their life history they spawn from

January to May in the Sargasso Sea; their pelagic eggs develop into leaf-like larvae—the leptocephali—which drift for $1\frac{1}{2}$ to $2\frac{1}{2}$ years in the ocean currents towards the continental shelves, and here they metamorphose into elvers which swim up the rivers and so into freshwater where as yellow eels they feed and grow. Several years later the adults leave freshwater and run down to the sea. While yellow eels are caught in coastal fisheries it is as silver eels that both species are believed to return to their parent spawning area, to spawn once, and then die.

This account of the life histories of the two eels follows that put forward by the Danish biologist Johannes Schmidt (*Phil. Trans. R. Soc. B* **211**, 179; 1922). Schmidt based his hypothesis on the size distributions of eel larvae taken in the North Atlantic by Danish research

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vessels and merchant ships. But spawning eels have never been caught in the Sargasso Sea and only four silver eels have been taken off the continental shelf of Europe. No eel eggs have been identified from plankton samples taken in the Sargasso or from any other area. Schmidt used the distribution of the smallest eel larvae taken by research and merchant vessels to define the spawning area for European eels as being bounded by latitudes 20°N and 30°N and longitudes 48°W and 65°W. Although the collection of American eel *leptocephali* was poor, he suggested that this species spawned in a contiguous area somewhat to the southwest.

Schmidt's hypotheses gained general acceptance and his major conclusions were not questioned until 1959 when the English biologist Denys Tucker challenge the "well-publicised saga" (*Nature* 183, 495; 1959). Tucker offered a "new and drastically different interpretation of the observed phenomena", namely that European eels do not return to the Sargasso spawning ground but perish in continental waters, that European and American eels are not two distinct species, and that eel populations in Europe are maintained by larvae from American eel parentage. This touched off a lively correspondence (*Nature* 183, 1405; 184, 1281; 1959; 185, 589; 1960) with eel workers critical of his new solution. Perhaps Tucker's major contribution was not so much a solution as a reminder that much of Schmidt's story was based on circumstantial evidence. But while most biologists are reluctant to reject the classical account, Tucker's hypothesis has never been disproved, and little weight has been given to Vladykov's point (*J. Fish. Res. Bd Can.* 21, 1523; 1964) that the presence of American eels in Trinidad and the Guianas in South America is only consistent with a spawning area much farther to the south of that suggested by Schmidt.

There are a number of outstanding questions to answer in the biology of the North Atlantic eels: are there two distinct species; and if there are, where are the spawning areas and when do they spawn; what are the mechanisms for species separation during the period of larval drift; and what are the migration routes and orientation mechanisms of eels in the open sea?

Renewed interest in the life histories of eels developed from the 'Symposium on eel research and management' sponsored by the International Council for the Exploration of the Sea (ICES) and the European Inland Fisheries and Advisory Council (EIFAC) and held at Helsinki in 1976. In part this is attributable to increasing commercial interests and pressure for management. The first sale value of

adults of the two species is about £10 million annually. They are often under-utilised, being processed for pet food in the United States, whereas when smoked they fetch £20 per kg on the Danish retail market and live unpigmented juveniles—'glass' eels—may fetch £500 per kg on the Japanese retail market. Furthermore there is concern for overfishing in some European rivers and the danger is thought to be real in areas where both elvers and adults are exploited. The Helsinki symposium endorsed a recommendation that ICES and EIFAC should organise an International Expedition to the supposed spawning area of the European eel. International support for the venture was enlisted and the Federal Republic of Germany has made the research vessels 'Friedrich Heincke' and 'Anton Dohrn' available for two cruises to the Sargasso Sea early in 1979 under the leadership of F.-W. Tesch of Hamburg. A reconnaissance cruise is now under way.

The possibility of UK support for the Expedition has been discussed by an *ad hoc* group set up by the Royal Society's British National Committee on Oceanic Research and by the Natural Environment Research Council. Although it was agreed that the 'eel problem' raised scientific problems of great interest and merit which deserved further study, it was thought that existing collections of larval material should be properly examined before launching an International Expedition and this view was sustained at a meeting of the ICES Planning Group held in October 1978. Attempts to secure support for an International Expedition have therefore been postponed until the results of the reconnaissance cruises are to hand and the study of existing collections completed.

Progress is being made with the material. All Schmidt's collections from west of 40° longitude, including much unpublished material, are being systematically examined and analysed by Jan Bøetius of Schmidt's old institute, Danmarks Fiskeri-og Havundersøgelser, Copenhagen, in collaboration with E. F. Harding of the Statistical Laboratory, University of Cambridge. Results from the collections of European eel material will be forthcoming but enough is already known to suggest that Schmidt lacked sufficient material to deduce the spawning area for American eels. Examination and analysis of substantial larval collections pertinent to the American eel is to begin soon at the University of Maine in cooperation with other American universities and the National Marine Fisheries Service. At the University of Maine a computer simulation study of larval drift is being developed for the western North Atlantic. The known freshwater distri-

bution of American eels may, in conjunction with these studies, allow a prediction of a likely spawning area for the American eel.

The return of European eels to the Sargasso could be shown directly by their capture at sea and Tesch might succeed in this aim in 1979. Methods other than direct capture are being considered for the future. Large predators may eat eels and their stomach contents could be examined; if the otoliths of adult American and European eels are distinguishable by chemical analysis, scanning electron microscopy or other means, the screening of bottom sediment cores might provide evidence of their distribution; and specific chemicals from mucus or the gonads of mature eels might be detectable in seawater samples, so allowing the presence of adult eels to be deduced without capture.

Nearly 20 years have passed since Tucker suggested that there was but one species of North Atlantic eel. A search has been made for genetically based biochemical differences in haemoglobins) serum proteins, and various enzyme systems. Chromosome number and types have also been examined. But the results of this work have not yet given conclusive evidence for species distinction.

The mechanism for species separation (or the eco-phenotypic separation of Tucker) during larval migration has been assumed to be simply drift from slightly different spawning grounds, differential growth rate and age-at-metamorphosis. That the separation mechanism is imperfect is demonstrated by the arrival, in small numbers, of American elvers in European waters. New collections of larvae to be made along transects into the southwestern North Atlantic during Tesch's 1979 cruises and the examination of existing larval collections and the larval drift simulation study should help to clarify the problem of larval separation.

As no migrating adult eels have been caught off the continental shelves, nothing is known of their migratory routes and nothing is known of their orientation in the sea. Ultrasonic telemetry tracking studies of the movements of migratory eels and displaced non-migratory eels are now being carried out in European continental waters by Tesch and by McCleave, Greer Walker and Arnold (at the Fisheries Laboratory, Lowestoft), and Tesch plans to follow hormonally-treated adult eels in the Sargasso Sea in 1979. But there is undoubtedly a long way to go before the migration of the adult eels can be described in behavioural terms. Nevertheless, the next decade should see a considerable advance in our understanding of the old 'eel problem'. □

SPRING BOOKS

R. A. FISHER was one of the really great scientists of this century, a man of immense ability, with an independent mind of great penetration combined with wide interests and striking erudition. He is, of course, best known for his contributions to statistics and genetics, but he was interested in any variable phenomenon and indeed made a basic statistical contribution to the study of palaeomagnetism, whose implications for the theory of continental drift he both recognised and expounded. All these he combined with a capacity for eccentric behaviour which, though sometimes exasperating or embarrassing to his friends, is now chiefly remembered for its contribution to the seemingly inexhaustable fund of "Fisheriana"—some of it no doubt apocryphal but most of it firmly based on fact. It has indeed been said that in later years, biometrical meetings not uncommonly developed into gatherings engaged in telling endless stories about the master.

I had the privilege (and it was a privilege, even if at times a little wearing) of working closely with Fisher, in the Galton Laboratory, for several years in the mid-1930s. They were unforgettable years, and not just because they saw the publication of that great book, *The Design of Experiments* (which Fisher gave as a lecture course while he was writing it), the introduction of such new statistical techniques as discriminant functions, the analysis

of Nabours' data from wild populations of grouse locusts which revealed selective differentials attaining the hitherto unheard of level of 10%, or even the birth of the serological unit which was to unravel the complexities of the *Rh* gene and do so much for the study of human blood groups. They were unforgettable too for the way in which he gave unstintingly of his time in

Life with Fisher

Kenneth Mather

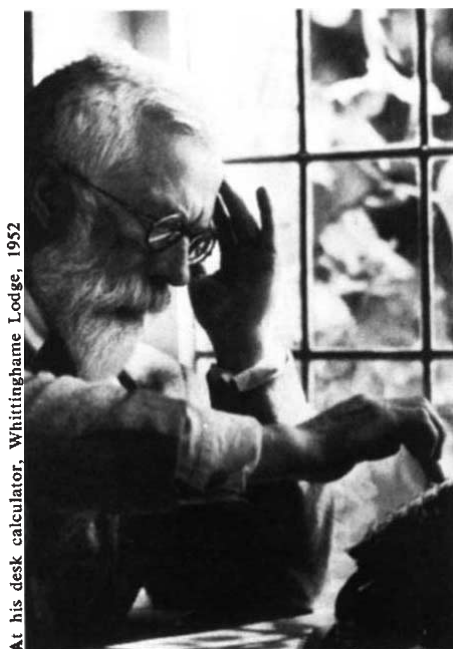
R. A. Fisher: The Life of a Scientist. By J. Fisher Box. Pp.512. (Wiley: New York and Chichester, UK, 1978.) £16.

explaining to a hitherto statistically uneducated young biologist how to approach the analysis of data, including the clarification of the (at any rate, to him) elementary mathematical points at issue, and for his insistence that genetical experiments should be designed with the same care as agronomical experiments, including the calculation of the laboratory resources that would be required. In the case of mouse experiments this was done in terms of mouse-cage-months, and Fisher often based his calculations on optimistic figures for speed and proli-ficity of breeding. One soon learned that outright argument about this would serve only to irritate him, but a continuing attitude of concern would lead him to accept a programme more realistic in its expectations, particularly as the numbers of mice failed to mount in the expected way. One learned, also, to look after snails, grouse locusts and dogs as well as mice, and even for one brief period to look after some marsupials which he imported from Australia on the grounds that marsupial genetics had been sadly neglected.

Life with Fisher was indeed a never-ending education. His kindness and patient helpfulness, especially to young people, was something that those of us who benefited from it will never forget. We also saw, however, how coldly and continually hostile he could be to those whom he suspected of being hostile to his views or obstinately misguided in their own. The mid-1930s saw too not only his last polemical

exchange with Karl Pearson but the sad controversy with W. S. Gossett ("Student") with its revelation of a contentiousness so stubborn that it did not even spare an old friend. This contentiousness no doubt sprung from the treatment he had himself received (though not from Gossett) in his earlier struggling years, and it was no doubt also to these early years that we should trace Fisher's deep-seated distrust of accepted authority to whose nature he refers specifically in the introduction to the *Design of Experiments*.

Our personal recollections of this outstanding, and in some ways paradoxical, man can now be filled out and extended by the biography which his daughter, Mrs Joan Fisher Box, has written. It is a lengthy book, but for me at least it made compulsive reading. It supplies us with a full account of his ancestry and early life; and with it we can enter into the life of the Fisher family, its excitements, its happinesses and its sadnesses in a way which is possible only when looking through the eyes of one of the family itself. One can share Mrs Box's nostalgia in looking back at her childhood, as in the brief but touching account of Christmas in the Fisher household, including the story of how on one occasion "the existence of Father Christmas was challenged and one little boy stayed awake and saw for himself the figure who entered and filled the socks; he wore an unfamiliar long gown of



At his desk calculator, Whittinghame Lodge, 1952



At the seaside, 1938

SUPPLEMENT

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At Lake Junaluska Assembly, North Carolina, 1956

scarlet silk, whose pink facings might have revealed to a more sophisticated watcher that Father Christmas was a doctor of science from Cambridge University; a mask with curling silver beard hid his features". This is true Fisher and a worthy addition to our store of Fisheriana. Nor does Mrs Box burke the sad story of the break-up of Fisher's marriage. She paints her father's picture in a way that would have met with Cromwell's approval—"warts and all": blemishes as well as greatness.

It would, however, be wrong to leave any impression that the book is wholly, or even mainly, devoted to Fisher's family life and personal matters. On the contrary Mrs Box presents a full account of his scientific life, his scientific interests and scientific achievements, with which the more personal things are interwoven, as indeed must have been virtually inevitable in writing about Fisher. The early years,

before 1919, not only bring in reference to the paper he gave while still an undergraduate in 1911, in which his interests in biometry and genetics are foreshadowed, but also cover his two early major works on the exact distribution of the correlation coefficient and the correlations between relatives on the supposition of Mendelian inheritance—the latter now a genetical classic.

He went to Rothamsted in 1919, initially for such time as a grant of £200 would cover, but stayed until 1933. During these fourteen years he recast the whole theoretical basis of statistics and developed the modern methods of designing and analysing experiments, while at the same time he was generating his theory of the evolution of dominance and writing his seminal book *The Genetical Theory of Natural Selection*—and this genetical work was all carried out in his spare time.

When he succeeded Karl Pearson at

the Galton Laboratory in 1933, he was able further to develop his genetical interests by undertaking genetical experiments on a sizeable scale and by founding the serological unit. But a wealth of statistical work continued, including the sharp defence of his basic approach against those who chose to criticise it. Much the same balance of interests continued in Cambridge where he succeeded R. C. Punnett as Professor of Genetics in 1943. The Cambridge days saw the publication of the *Theory of Inbreeding*, arising from some of his mouse experiments, and also *Statistical Methods and Scientific Inference* in which he discusses at length the problems and logic of inductive inference. They also saw the flowering of the serological unit and the recognition by many honours from universities and academical bodies in a number of countries, as well as by the knighthood conferred on him in 1952. He travelled widely, especially in the USA and India where his ideas had a great influence, and after retirement in 1958 his Australian friends persuaded him to make his base in Adelaide where he died in 1962.

Courtesy Mrs Horace Norton

Each period of his life can be seen as marked by its distinctive scientific achievement, springing from the problems which his various appointments posed and the opportunities which they offered. It is a fascinating story which Mrs Box has to tell, and she tells it well. Her presentation and assessment of his work is comprehensive, balanced and informative, and she points to limitations where she sees them, as for example in Fisher's much smaller interest in the direct study of the hereditary materials than in the numerical relations to be found in breeding experiments.

This is indeed a fascinating book. To those who knew and worked with him it will bring the special pleasure of recalling a past association that was both memorable and eventful. To a wider audience it offers an account, at once informative and human, of a great scientist who gave us statistics as we now know it, while at the same time establishing himself among the great leaders of genetics especially in the study of natural selection and its impact on populations and their properties. □

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Morgan's crisis

C. D. Darlington

Thomas Hunt Morgan: The Man and His Science. By G. E. Allen. Pp. 447. (Princeton University Press: Princeton, New Jersey, and Guildford, UK, 1979.) \$31.50; £15.80.

DURING his life the work of Thomas Hunt Morgan was the centre of many disputes. The chief of these concerned the validity of his famous discovery: Were the chromosomes the physical basis of heredity? Others concerned secondary questions: What did the discovery mean? How was it made? And who made it? The first question for long divided the scientific world into battling parties. The others divided only the new believers who in the course of 50 years have taken possession of the battlefield. For them the secondary questions have grown in importance. And for their solution the life of this enigmatic man has become of absorbing interest.

We now have two books on Morgan from outside his scientific circle which help us to answer these questions. One is a concise but accurate and convincing account by Shine and Wrobel (Kentucky University Press, 1976). The other is this larger treatise by Allen. Neither comes to grips with the crucial issues, perhaps because no one can who was not himself in the fray. Before it is too late, therefore, we should recall how those issues came to a crisis as they did for Morgan in the spring of 1910.

It was the meeting of two apparently conflicting ideas which created this crisis. Both had come from Europe to the United States in the course of Morgan's teaching life. They had been brought by two unconnected missionaries. One of these was E. B. Wilson who had become the head of Morgan's department at Columbia University. The results of his visits to Europe in 1883 and 1892 were concentrated by Wilson into the three editions of his great book, *The Cell*; a book which cost him the labour, not of one decade (as Dr Allen supposes), but of a lifetime. It was a book which argued the central position of the chromosomes in heredity and development.

The other missionary was Bateson, whose journey from England was different in period, scope and direction. His Silliman lectures in 1907 had argued the central position of Mendelism in the study of heredity. And they had converted to his view several shrewd critics at Yale and at Harvard, notably East on the plant side and Castle on the animal side.

So two schools, sharply divided, came into being in the United States. Neither seriously converted the other. And neither in the least converted Morgan. Indeed it seemed that 20 years of embryological experiment had left Morgan without any theory of his own and ostentatiously sceptical of everybody else's. He doubted Wilson's chromosomes. He ridiculed Bateson's factors. He confused Darwin's origin of species with his natural selection, which he even condemned as teleological. And (to use Allen's word) he "chastised" Weismann for his errors. He paid some respect to the mutation theory of de Vries. But when in 1907 Fernandus Payne, his pupil jointly with

Wilson, asked him for a problem he gave him the Lamarckian task of breeding Castle's *Drosophila* in the dark for 60 generations.

Payne, as he recalled 67 years later, failed to diminish the fly's eyes by two years' life in the dark. But Morgan himself, in the third year, with a hand lens, was able to find something in the light, a mutation, a single male fly with white eyes. The factor responsible proved to be, as it was likely to be, in the unpaired sex chromosome. From this one fly Morgan could therefore with little trouble demonstrate Mendelian segregation, sex-determination by chromosomes, and sex-linkage. Even, if he had thought of it, he could have seen a ratio distorted by natural selection. The progeny of this one fly corrected unmistakably all Morgan's recent mistakes.

In this embarrassing position Morgan quickly wrote a letter to *Science*. Then on 7 July, 1910, writing from Wood's Hole (although Dr Allen does not seem to know) he sent the much corrected typescript to Bateson at Cambridge, offering to share with him his stocks and the theory they might carry with them. Bateson, however, had come to distrust Morgan's judgement and he evidently declined the stocks; he also certainly rejected the theory.

Having failed to enlist Mendelian support, Morgan, back in New York, turned to the chromosome side, to Wilson in the next room to his own. Wilson was able to tell Morgan about the Belgian cytologist, Janssens, who from seeing chiasmata between chromosomes at meiosis recalled, as German cytologists had done earlier, Weismann's theory of meiosis. This theory demanded recombination which would arise by "crossing over" and would explain linkage. Wilson was also able to recommend to Morgan three pupils who had learnt about the chromosome theory from him, and about Mendelism from the books of Bateson's disciples, Punnett and Lock. They would breed his flies.

The results in the fly-room were dramatic. But what exactly happened? The answer I partly discovered for myself when twenty years later I questioned the two leading figures in the drama. Who did it, I asked Wilson; and his reply was quite firm. Morgan's three greatest discoveries, he said, were those three men, Bridges, Sturtevant and Muller. Then I asked Morgan what Janssens' idea had meant for him. Janssens, Morgan replied, being a Jesuit, had seen his problem not scientifically but teleologically. His idea of crossing over must have come from the belief that there could not always be four products of meiosis in all sexual reproduction unless all four were different. Morgan clearly had not read

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Janssens' article. He did not know that in his first sentence Janssens deduced his argument from Weismann, that is, from the belief that only with recombination could natural selection be made to work. I sadly concluded that Morgan had never understood, and perhaps had long made up his mind not to understand, the chromosome side of the chromosome theory.

The gaps in understanding between Wilson, Morgan and Bateson were profound. How profound is shown by their continuance today, and by the failure of Morgan's biographer to notice them. For this reason he does not realise that the process of crossing-over between chromosomes has never been seen in female *Drosophila*. He does not realise that it remains, as Morgan would have said before he inferred it, a pure speculation. Nor does Allen realise the fear Morgan had of discussing the other side of the story, the absence of crossing over in the male *Drosophila*. Was its purpose perhaps to make his experiments possible? We shall never know.

For over 20 years Morgan had demanded facts and denounced speculation. But now, swept forward by the success of his clever young men, he found himself inscribing his name as the head of a grand speculative movement. Dr Allen devotes proper attention to those three young men: Bridges, the quiet easy-going countryman from the north, whose keen eyes discovered hundreds of mutations;

Sturtevant, the observant horse-breeder from the south whose ingenious experiments and interpretations carried him beyond heredity into development; Muller, the intense excitable New York Jew whose imagination went too fast and too far for Morgan's understanding or forbearance.

These men did not create a unified genetic theory. That was too much to expect. But they put together the assembly line of a great *Drosophila* industry. Surely no city but New York at that time could have brought together such a combination of talent. And if Morgan had a "creative genius" as Dr Allen calls it, it was that, when he saw what they could do, he let them do it.

The defect of this *Drosophila* industry lay in the limitations of the fly. It failed to reveal the chromosome processes underlying its heredity and indeed heredity in general. The diosophilists had to avoid meiosis. Making a virtue of necessity, Muller therefore quickly went beyond the chromosome to the gene. There the reluctant Morgan followed him to adopt the gene as his own. The chromosomes could then be taken for granted. Reversing the order of Nature the chromosomes did what the fly required. This was Morgan's "Theory of the Gene".

After the lapse of years Morgan, having lost interest in the experiments of genetics, and never having sustained an interest in its ideas, was awarded a Nobel Prize. Dr Allen's account allows us a glimpse of the embarrassment that once again overtook him. Having delayed his journey to Stockholm, on his way he arrived in Edinburgh still uncertain what, and whom, and how much, he should acknowledge. Such a failure to sort out his intellectual antecedents might be thought to be a merely personal problem and one not uncommon in science. But in this case, its effects have survived far beyond any personal interests. The hiatus in the relations of Morgan, with Bateson on the one hand, and with Weismann, Boveri, Janssens and Wilson on the other, has left its traces in the dichotomy of genetics as it is taught today, a defect liberally illustrated in the present biography.

The Nobel Prize had been heralded by a last general discourse, a book of lectures unhappily entitled *The Scientific Basis of Evolution*. In the one original chapter in this work Morgan finds himself trapped with the too obvious notion that the evolution of man introduces a new biological principle. In his view there are now "two processes in inheritance" in man. The inheritance of culture, he explains, is now independent of the old limitations of genetic inheritance as under-

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stood, for example, in the fly.

It is surprising that Julian Huxley in a book on evolution which he dedicated to Morgan should have adopted this fallacy of Morgan's 30 years later as his own. Without apparently knowing its source he described it as "psychosocial inheritance". But it is even more surprising that Morgan's biographer, claiming to be a Marxist (who should always know what he is going to discover), has now failed to notice this gem of Marxist dialectic: for here is an idea which opens the way so agreeably to Lamarckism and Lysenkoism. From it the whole froth of our social sciences today, with their specious arguments and calamitous consequences, easily flows and overflows.

Dr Allen must have occupied many years in preparing this work, as his method of assembling it is a recipe for chaos, chaos in writing and chaos in reading. His accommodation of Bateson (and the present reviewer) at the John Innes Horticultural "School" which he places in Merton College, Cambridge, is an extreme example. But what can be expected of a writer who buries his references irrecoverably in 743 footnotes dispersed throughout the text? Clearly his book may help later enquirers into the history of genetics. But not unless they are willing to start at the beginning again, however distressing the results of their enquiries may be. □

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Courtesy Dr Tove Mohr

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Master of biological research

M. R. Pollock

Selected Papers in Molecular Biology. By Jacques Monod. Edited by A. Lwoff and A. Ullmann. Pp. 753. (Academic: New York, San Francisco and London, 1978.) \$29.50; £19.15.

No doubt there will always be some scientists, overburdened by the appalling mass of literature that has to be sifted through these days, to protest against the publication of reports that have already appeared in print and are generally available in libraries. But there must be many more who will warmly welcome this collection of papers by Jacques Monod, who was unquestionably one of the greatest of the architects and exponents of the new biology. It is, in fact, a selection of less than half his total output of 131 scientific papers and, rather unfortunately, does not include any of his forewords, essays, reviews or recorded interviews on broader themes, which are less easy to get hold of than the others. Indeed, there must be many who would have liked an unexpurgated collection of *The Complete Works*. He was a master of beautiful and clear writing, both in French and English, and his views on almost everything are worth reading.

Presumably the basis of the editors' selection was to avoid redundancy of material and the extravagance of two volumes. The 62 papers reprinted make a fair-sized book which, priced at over £19, seems expensive enough for something in principle already available, but still less than half what would have to be paid for loose xerox copies. The volume is generally well produced and only relatively few articles have had to be reduced to an extent where the size of print is borderline for easy reading. A fair proportion of the papers excluded are abbreviated preliminary reports appearing in *Comptes rendus* . . . , completed versions of which are published elsewhere; but there are some apparent inconsistencies. An introductory explanation of the selection principles used would in any case have been interesting and reassuring.

The book in itself is a great tribute to the work and genius of Jacques Monod. "Occasionally the career of a scientist", write the editors in their preface, "is marked by an important discovery. It is most unusual that it be

illuminated by an uninterrupted series of great discoveries and still more unusual when each discovery gives rise to new concepts and opens new vistas". But it is much more than just a tribute. It is a story, told in the dry, restricted language which research workers are expected to use for recording what they have done and how they think, of one man's contribution to scientific knowledge. And, apart from a very few pieces of work mostly at the beginning of his career, it is the story about a single problem: the specific control of enzyme production in living organisms. This problem originally presented itself, apparently rather narrowly, as the phenomenon of 'enzyme adaptation' in microorganisms and it was tackled by Monod mainly through the

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study of a single system: the famous β -galactosidase of *E. coli*; but it carried with it tremendous implications for a better understanding of the wider problem of the phenotypic expression of genetic potentiality in general.

But heaven forbid that anyone should read the book like a story from beginning to end! It is, rather, a book of reference and inspiration, to be consulted by research students in order to learn something about how a master of biological research thought and worked, and for historians to understand better thereby the evolution of ideas in one area of biology covering less than 30 years: a period, however, that comprised the profound revolution in our thinking in genetics, biochemistry and ultrastructure which we now call molecular biology.

This period in Monod's work

stretched over only 20 years: from the late 1940s, during which he wrote his first long classic review on 'enzymatic adaptation', published in *Growth*, to his Nobel prize article of 1966. To get a proper perspective, it is illuminating to read these two articles directly in sequence. In the first, the problem was defined, the essential questions posed and some tentative, plausible mechanisms (often incorrect, but that is not the point) suggested. In the last, most, if not all, the questions had been answered.

In between, Monod had master-minded developments crucial to providing the answers. These include (amongst others) the discovery that specific inducers of enzyme formation are not necessarily even ligands of the enzyme, let alone substrates (1950); the illuminating application of the 'allometric plot' to the study of the kinetics of enzyme induction (1952); the demonstration (1954) that enzyme induction involved *de novo* biosynthesis of the specific enzyme protein from amino acids; specific 'permeases' (1955); and the discovery (1958) that the inducer operated by antagonism of a specific gene-determined repressor. Thereafter followed the 'presentation' of a veritable avalanche of new concepts: 'structural' and 'regulatory' genes, the 'operator', the 'operon', the 'promoter', and finally (1962) the property of allostery which is of fundamental importance to the understanding of the control of enzyme function and enzyme synthesis.

In nearly all these developments Monod played a vital—usually a completely dominant—role, although it is important to realise that outstanding colleagues such as Mel Cohn and François Jacob (and indeed several others whom he guided and stimulated in his inimitable fashion) often also played an important—and sometimes essential—part; not forgetting André Lwoff himself who provided, right from the beginning, the emotional, intellectual and material background support without which almost everything would have been five times more difficult.

Monod's essential talent lay in his ability to combine a precise, logical formulation of a problem (that is, in terms that could be tackled experimentally and had meaning within the current 'paradigm') with an imaginative grasp of its implications in biology as a whole, and the power to initiate plausible interpretations yet move on from a concept, however attractive, once it was beginning to obstruct, rather than promote, the development of a subject. The difficulty, of course, is to recognise the obstruction. It is failure to do this that so often prevents research workers from making original discoveries. And nowhere is this better illustrated than in Monod's final rejection

Jacques Monod

Camera Press

tion of the cloying, but once attractive, hypothesis that the close similarity between the specificities of enzyme induction and enzyme function must mean that the mechanism of induction involves combination between the inducer and a receptor molecule consisting at least of a modified version of the enzyme, if not the enzyme itself. But the essential point to be grasped was that the relationship is functional, not chemical. Once Monod had discarded the false implications of the specificity similarities, he was free to

postulate the true nature of the receptor, characterise and define the *lac* repressor and discover the phenomenon of allostery.

Thus, almost more than from the actual discoveries themselves, we can benefit from studying the way he used his mind. And that is one of the chief reasons why this collection of masterpieces is so valuable. □

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Strategies of dissent

Vera Rich

To Build a Castle: My Life as a Dissenter. By Vladimir Bukovsky. Pp.352. (André Deutsch: London, 1978.) £7.50.

In 1961, Vladimir Bukovsky was a biology student in Moscow. Today he is a biology student in Cambridge. *To Build a Castle* relates the history of the intervening years, which, with a few brief intervals of relative freedom, were spent in the prisons and psychiatric hospitals of the USSR.

This period spans the rise of dissent and opposition in the USSR. One curious feature of the dissent movement is that, at least in Russia proper, scientists played a major role, and at the beginning, the leading role. (In the non-Russian republics, such as Lithuania and Ukraine, other factors such as national aspirations make the picture more complex.) At one end of the dissent spectrum, leading scientists could defy the régime from a position of strength, knowing that the state needed their expertise; at the other, it was among the students of the natural sciences that latent doubts began to take a more concrete form—total ideological conformity and commitment being ensured in the humanities and social sciences by the preliminary screening of University entrants.

Bukovsky's book outlines how such doubts could grow and crystallise in a student living in the immediate post-Stalin era. After his expulsion from the University in 1961, we follow him through the long routine of arrests and incarcerations. This book is not, however, just another set of labour-camp memoirs. The *Castle* of the title is an imaginary one, which Bukovsky patiently constructed in his mind through long years in prison and psychiatric ward, as a means of

therapy and self-protection against the grim reality which surrounded him.

Accordingly, the main interest in the book is to be found in what may be called the strategies of dissent. Nowadays, it is taken for granted that any civil rights protest is essentially a demand that what is guaranteed by the constitution should be implemented in fact. Zhores Medvedev, the geneticist, for example, waged a major campaign against Soviet censorship on the grounds that "Secrecy of correspondence is guaranteed by law". Bukovsky relates how this procedure was first developed in the late 1960s and how, when Alexander Yesenin-Volpin, the mathematician, first urged it, many budding dissidents were sceptical of its potential value—a scepticism which, as Bukovsky describes, has been refuted over and over again in the past decade.

Most fascinating, however, are the chapters dealing with Bukovsky's experiences of mental hospitals, for here we have the first-hand background to the reports which Bukovsky, in a brief spell of freedom, managed to transmit to the West. These reports for the first time drew attention to the new Soviet theory that political discontent equals mental disorder. Here too is the practical experience of psychiatric examinations and medical "commitments", which led Bukovsky, in collaboration with psychiatrist Semeon Gluzman, who is still serving a seven-year sentence in a labour camp, to write their famous *samizdat*, *Manual of Psychiatry for Dissidents*. This is a slim but detailed handbook of how to behave under interrogation, with some pungent analyses of the main psychological types of interrogator likely to be encountered, and the best strategies for circumventing and frustrating them. In a book of the stature of *To Build a Castle*, it is regrettable that space could not have been found to include the 'Manual of Psychiatry' as an appendix—this is, however, one of the few faults one can find with an otherwise excellent work. □

Vera Rich is Nature's regular correspondent on Soviet and East European affairs.

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Eye-opener on life

A. J. Cain

Life on Earth. By D. Attenborough. Pp. 319. (BBC/Collins: London, 1979.) £7.95.

THIS pleasant book is not, and was not intended to be, anything like a scientific text. Although based on the highly successful television series, it is also far from a mere coffee-table selection of excellent pictures. In character, it probably approaches most closely Kenneth Clark's *Civilisation*. To quote from the Introduction, the purpose in making the films was "to try and survey the whole animal kingdom and consider each great group of animals in the light of the part it has played in the long drama of life" and in order to do so on a practicable scale "to try and perceive the single most significant thread in the history of a group and then concentrate on tracing that, resolutely ignoring other issues . . .". To do so, one must know something about the anatomy and physiology of the basic design in each group—"The quintessence of, for example, a lizard is only fully understandable in the light of the particular possibilities and limitations dictated by its reptilian nature, and that, in turn, only becomes comprehensible in the light of its past."

David Attenborough has never recovered from his first experience of the tropics, and he is quite right not to; no one, including professionals, is really a biologist until he has experienced personally the incredible richness, the diversity, the multitudinosity of life where it really burgeons. And when he has, it is an experience he knows will be valuable to everyone, not only professionals, if it can be communicated. This book will be for very many people, especially young ones, literally an eye-opener on life; but it will be much more, because there is a definite theme, or rather several, running through it. It is the welcome antithesis to those woolly-mammal-pretty-girl books that bored everyone some years ago in the name of conservation.

It is based, therefore, as far as possible on living forms—indeed, one of the surprises of the television series is that while taking full account of time, and sufficient outstandingly well-preserved or interesting fossils, and of plate tectonics, it goes extremely lightly over our old friends the dinosaurs and other extinct reptiles, and most of the extinct mammals. For the purposes of

the book, this is probably right—far more can be said accurately of the anatomy, physiology and ecology of living things, and there is a rich array of 'living fossils' to draw on. Occasionally, this approach seems to bring a weakness of treatment; if one wishes to show the extraordinary continuity of groups, even among the vertebrates (which after all are a fairly compact group), then the beautiful sequence from early crossopterygians to early reptiles should surely not be omitted. To jump from lungfish and coelacanths to modern amphibians with only a passing mention of *Eryops* leaves one gasping in a very rarefied atmosphere. But a rigid exclusion was obviously necessary, and it is ungracious to pick out omissions.

One should be grateful, in fact, for how much there is in the book. Hardly any 'living fossil' of importance is omitted, and most get considerable discussion. (The only one I wished had not been left out is that tropical plant that so closely parallels the fossil land-plants (psilophytes) of the Rhynie Chert.) In each case, the present mode of life is discussed with expert knowledge and assistance. Every time, each group is related to the state of the Earth when it arose, the nature of the fauna and flora it had to live with, and what it did or does that others did not or could not do.

Inevitably there are many gaps in our knowledge, and the author is rightly insistent on them. Why do so many groups of invertebrates acquire fossilisable hard parts at the beginning of the Cambrian? Was it really a sudden cooling of climate that extinguished the large land-reptiles (and many small ones) at the end of the Mesozoic? What exactly is the evolutionary relationship between molluscs, arthropods and annelids? As against that, the author gives an excellent picture of life before the oxygen-shield developed, and living things could safely get out on to land. He points out the enormous advantages of being a reptile (you can live on one-fifth of the food necessary to keep an equivalent mammal stoked up), as well as the inevitable disadvantages.

All through, he avoids the fallacy of believing that a primitive form is necessarily inefficient; normally it is efficient enough for a particular mode of life. He is especially interesting in this connection on the marsupials, where, in fact, he seems to miss a point. The radiation of marsupials in South America in the Palaeocene seem to have taken place in the presence of eutherian mammals. So, far from being extinguished as more primitive and inefficient, they seized the ground insectivore, arboreal nocturnal insecti-

vore and carnivore modes of life so successfully that they held them to the exclusion of the South American eutherians. These took up all other possible modes, and the combined fauna did not become largely extinct until 50×10^6 yr later, when very different eutherians came in from the north and both marsupials and local eutherians suffered badly. Equally, there are numerous endemic eutherians in Australia today, coexisting with the marsupials and making up a complete range of fauna. With the ruin of their habitats by cultivation, both sorts become extinct.

Inevitably, some specialist will quarrel with almost any sentence of the book, but what is so useful is indeed the breadth of treatment and the insistence on living things as living things, solving particular problems of existence. Many of the points raised will stimulate naturalists to much further thought. It has been known for centuries that cormorants and darters get their plumage soaked under water, and have to sit out in the sun, drying themselves out. A survival of a primitive character? Possibly, but why should it survive? If every other sort of aquatic bird has evolved waterproof clothing, why can't they? But if, as suggested, the air trapped in this waterproof clothing makes the bird buoyant and it has to struggle to get down under water, does this mean that although cormorants and other pelecaniforms can stay under for only a short time, they are more adept in catching fast fish? If so, then the lack of air in the plumage may be a secondary adaptation, specially selected for. After a while, one begins to wonder if any character is merely primitive, merely there because one's ancestor had it. Is it not kept because it still has a use? The author comments on the retention by modern ratites (ostrich, emu, rhea, etc.) of adaptations for flight, such as air-sacs in the bones. Possibly when their ecology and physiology are better understood we shall see why they didn't convert their legs to horse-like ones. Whereas in the nineteenth century so much had to be explained away as the retention of ancestral characters, much now makes sense when we know how exactly an animal lives.

Coming to the book after the television programmes engenders mixed feelings. One is glad to have the book as a permanent record, and still more because it is extremely well written and enables one to grasp the themes of the whole series. The text is clear (I noted only four misprints—and one transposed caption); the price is low. The illustrations are superb, and only a very few are spectacular without much biological content (I noted the Goliath

beetle as one). But one is constantly reminded of others in the series—that wretched lizard cooling its feet in almost ritual attitudes, the parachuting frog, the toad's anti-snake attitude, the skimmer skimming, the caves of Naracoorte, Dinosaur National Park, the lancelets feeding, and scores of others.

Please may we have soon a de luxe edition with twice the number of plates, a millionaire's edition with four

times the number of plates, and a nationally assisted edition with ten times the number? Its value for schools alone would justify the expenditure of government money, and help to assuage the feelings of those who now have to teach spelling in the Universities. □

A. J. Cain is Derby Professor of Zoology at the University of Liverpool, UK.

Darwin's legacy

C. M. Yonge

The Beagle Record: Selections from the Original Pictorial Records and Written Accounts of the Voyage of HMS Beagle. Edited by R. D. Keynes. Pp. 409. (Cambridge University Press: Cambridge, 1978.) £30.

HARDLY to be realised by that monarch or indeed by any of his subjects, yet—viewed in the light of what was to come from it—the voyage of *HMS Beagle* could well be considered the most momentous event during the short reign of William IV. That vessel departed on her circumnavigation of the globe 18 months after he came to the throne and returned less than a year before his death and the accession of Queen Victoria. The numbers of those aware of the course and significance of this voyage have recently been vastly increased following the broadcast of the imaginatively impressive television series by BBC-TV.

Putting aside for the moment the eventual consequence in *The Origin of Species*, there was early appreciation of the immediate achievements of the voyage. Darwin's original account of what he saw and did was initially somewhat hidden because it formed volume 3 of the *Narrative of the Surveying Voyages of HMS 'Adventure' and 'Beagle' between 1826 and 1836* published in 1839. This appeared before a wider public six years later as the *Journal of Researches into the Natural History and Geology of the Countries visited during the Voyage round the World of HMS 'Beagle' under the Command of Captain FitzRoy RN*, which a reviewer in the *Quarterly Review* was to describe as "One of the most interesting narratives of voyaging that it has fallen to our lot to take up, and one which will always occupy a distinguished place in the history of scientific navigation".

How fortunate it was, one may well reflect, that the prime concern of the

voyage was a survey of the southern shores of South America. As a naturalist attached to a naval vessel, Darwin might have gone to the Antarctic as Joseph Hooker did, to Australia and New Guinea as Huxley was to do, or perhaps not further than the eastern Mediterranean, the limit for Edward Forbes. But South America, not long freed from restrictions on scientific enquiry only penetrated by Humboldt, was a different proposition. To the prematurely judicious eye of Darwin, only 22 when the voyage began, this continent presented so very

however, with products of later years at Down House, would have been no more than the work of an outstanding naturalist had it not been for the publication of *The Origin of Species*. Darwin then changed the direction of human thought and based his demonstration of the fact of evolution on "certain facts in the distribution of organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent". From that date all that Darwin had done and thought becomes of profound interest—above all his activities during the voyage of *HMS Beagle* which ended on November 7, 1836, "having completed the unusually long period of five years and one hundred and thirty six days" in commission.

Apart from the *Journal of Researches*, his granddaughter, Lady Barlow, edited *Charles Darwin's Diary of the voyage of HMS Beagle* in 1933 and twelve years later unpublished letters and notebooks in *Charles Darwin and the Voyage of the Beagle*, with



Two Fuegians fishing from a canoe

Drawing by Conrad Martens

many matters for contemplation—recent earth movements, a wealth of unusual animals and plants, above all of fossil remains—during the long process, with constant re-checking, of FitzRoy's surveys. There was the final bonus of the parting visit to the Galapagos Islands before the long journey home.

After return the first scientific outcome was *The Structure and Distribution of Coral Reefs*, the subsidence theory a result more of his previous observations of earth movements in South America than of those on the few coral formations encountered in the Pacific and Indian Oceans. All,

his letters to Henslow (1831–1860) in *Darwin and Henslow: The Growth of an Idea* in 1967.

The present, and most impressive, volume came initially into being—Professor Richard Darwin Keynes, a great grandson, informs us—after a chance encounter with Dr Armando Braun Menendez at Buenos Aires in 1968. His rich collection of books, charts and pictures dealing with the exploration of the southern hemisphere was found to include two of the sketch books of Conrad Martens. Darwin may introduce him; writing to his sister Caroline from Monte Video on November 13, 1833, with initial

reference to the first official artist, he states: "Poor Earl has never been well since leaving England and now his health is so entirely broken that he leaves us—& Mr Marten, a pupil of C. Fielding & excellent landscape drawer has joined us".

Such indeed were his qualifications as revealed by the great majority of the illustrations, many in colour, that adorn this book. They mainly depict scenes around the southern coasts because, after nine months, Martens left at Valparaiso, FitzRoy without money to employ him longer. But in these beautifully reproduced sketches of Port Desire, in the Beagle Channel, at the Santa Cruz River, at Chiloe and elsewhere, we view scenes that Darwin and all aboard HMS *Beagle* must have gazed upon. The few pictures not by Martens include John Gould's painting of *Rhea Darwinii* and FitzRoy's sketch of Coquimbo in Chile, with the simple drawing of Napoleon's tomb at St Helena by Darwin's servant Syms Covington.

Saga of a research vessel

George Deacon

On Almost Any Wind: The Saga of the Oceanographic Research Vessel "Atlantis". By S. Schlee. Pp301. (Cornell University Press: Ithaca and London, 1978.) £10.50.

THE *Atlantis* was built for the newly founded Woods Hole Oceanographic Institution in 1930–31, where, to begin with, most of the work was done in summer by scientists who held teaching appointments. Nevertheless the Trustees recognised that progress required a moderate sized ship, with reasonable living quarters, able to work in the deep oceans. H. B. Bigelow, Director of the Institution, and Columbus Iselin, general assistant and future master of the ship had much to do with her specification. She was a steel auxiliary, rigged as a ketch, with fore and aft sails, engined by a 280 hp diesel engine. Sail was chosen for economy, long range and steadiness, though it has often been suggested that yachtsman's preference for sail had something to do with it. It was, perhaps, something of an anachronism from the start. UK oceanographers had proved Captain Scott's square-rigged, coal-fired *Discovery* unsuitable for making widespread observations in

The skilfully edited text, aiming at a general narration of events, is a judicious blend of extracts from the *Diary*, with all of Darwin's letters to his father and sisters, together with those, largely on scientific matters, to Henslow. FitzRoy is represented by extracts from the second volume of the *Narrative* with letters, both official and personal, to Captain Francis Beaufort, Hydrographer to the Navy and under whose instructions the voyage was carried out. There are concluding passages from the *Narrative* in which he defends the biblical story with the record of the rocks—potent evidence for evolution in Darwin's hand—all explained as a result of the flood!

Charles Darwin has been well served by his family—first by his sons, then by his granddaughter; and now a great grandson has produced the culminating tribute. □

Sir Maurice Yonge is Honorary Fellow in Zoology at the University of Edinburgh, UK.

reasonable time, and had replaced her by a fully-powered vessel built for the same price as the *Atlantis*, though costing twice as much to run. But the *Atlantis* proved ideally suited for pioneering work with new techniques and ideas, and it was not until post-War expansion, involving multi-ship operations, and widening international activities, extended and tightened the work load that "slogging across the ocean at a walking pace" proved unacceptable. As time went on she used her engine more and sailed less, till in 1960, when her tail-shaft broke—after post-war fitting of a more powerful engine—she had to call for a tug.

By 1935 she had greatly extended

our knowledge of the water circulation in the north-western Atlantic Ocean and Caribbean Sea, of daily plankton migrations, nutrient cycles, bacterial populations in sediments, the Gulf Stream, Sargasso weed, and CO₂ in atmosphere and ocean, among other basic problems. She went on to make extensive biological and physical studies of the Georges Bank area. She had also taken part in the development of a large quantitative plankton net with a triangular opening, with 50 ft sides, and the Leavitt release gear for opening as well as closing. She began seismic exploration at sea with Maurice Ewing "who was constantly banging on the Institution's door" and was used for development of more efficient core-samplers and the bathythermograph.

She was laid up when America joined the war, and afterwards used extensively in studies of underwater acoustics and underwater explosions. Her coverage was extended to the mid-Atlantic ridge, the Mediterranean Sea, the South Atlantic, the deep trench west of Chile and Peru, and to the Red Sea which proved a severe test of her suitability. There were fairly hectic times when she cooperated with the *Vema*, Ewing's own converted yacht which is still at work though without her rigging. Progress was most rapid when specialists of different kinds worked together. "Hersey's gang . . . charged across the frontiers of acoustic technique and instrumentation . . . like an institution within the Institution". Cooperative studies of the Gulf Stream were also very effective.

Schlee's book gives a lively and realistic account of life at sea in a small ship, based on the journals of her scientists and officers as well as the Institution's archives. She was often uncomfortable, "most of one's existence is passed exchanging one small discomfort for another". Shortage of funds was an obvious as well as concealed cause of trouble. She had a deep-sea winch from the start and a precision echo sounder in 1953, but no powered windlass or freshwater showers till 1954. She is not the only small ship in which continued discomfort has led to bother at sea and trouble in foreign ports, but she had her share.

In 1957 most of the trans-Atlantic sections were worked by a larger fully-powered ship the *Crawford*, soon joined by the *Chain*, and in 1963 by the *Atlantis II*. The old 'A-boat' was going down hill, but still used for tasks that she could do as well as and sometimes better than larger ships. In 1966 she was sold at a nominal charge to the National Council for Scientific Investigation in Argentina. She was re-

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fitted in Boston "such care had not been lavished on the vessel since the end of World War II".

When she entered Woods Hole for the last time all the town turned out to greet her. She could not have continued so long or done such good work if long-serving captains and crew had not taken continuing pride in her, their unusual spirit and enthusiasm more

than making up for her old-fashioned design. She is still working from Buenos Aires, mainly for training cruises.

Her fine story is well told. □

Sir George Deacon was Director of the former National Institute of Oceanography. He joined the RRS Discovery on 28 January, 1979, for further study of the Antarctic Ocean.

Varnished tale of Scripps

Edward Bullard

Scripps Institution of Oceanography: Probing the Oceans, 1936 to 1976. By E. Noble Shor. Pp. 502. (Tofua Press: San Diego, California, 1978). Paperback \$5.95.

THE Scripps Institution of Oceanography was founded in 1903; in 1967 Helen Raitt and Beatrice Moulton published a history of its first fifty years. The present book starts in 1936, when Harald Sverdrup became Director, and brings the story up to 1976. It is a truly remarkable story. How did a small marine biological station grow to be the biggest oceanographic institute in the United States and, perhaps, in the world and then, in the 1960s, spawn a new campus of the University of California? The story is largely that of the three great Directors—Harald Sverdrup, Roger Revelle and Bill Nierenberg.

Sverdrup saw that the future of Scripps lay beyond the coastal waters of California. He obtained a ship, the beautiful schooner *E. W. Scripps*, built for racing and pleasure cruising, and converted her to a research vessel. It was Sverdrup who made Scripps a real oceanographic institute.

At the end of World War II he returned to Norway and, after a period of transition under Carl Eckart, Revelle became Director. Revelle had learnt during the War to 'think big'. He started a frantic race to raise money, hire people and build buildings. It was he who made Scripps larger and better and more fun than any other oceanographic institute. After a few years he grafted on to it a graduate school of science and engineering and turned this and Scripps into the University of California at San Diego, with himself as acting Chancellor. His strategy was original and brilliant. Most universities start with under-

graduate teaching and then, years later, develop a graduate school. This ensures a start with a rather pedestrian staff. Revelle's scheme enabled him to engage a prestigious staff from the start: among his early captures were Harold Urey and Maria and Joe Meyer. When the time came to choose a Chancellor for the new campus the Regents did not appoint Revelle. In 1964 he left California to become Director of the Population Studies Institute at Harvard.

It is a dramatic story and Betty Shor has told the part that concerns Scripps in meticulous and well documented detail; but somehow the realities of what happened have slipped away. Everything is so smooth, everyone behaves so well, everything succeeds, there are no rows and only the merest hint of the struggles that raged before Revelle was made Director and during his unsuccessful attempt to become Chancellor. We are told the names and deeds of all kinds of rather minor figures—for example, of the lady who presides so charmingly and efficiently over the aquarium bookstore—but the name of Edward Pauley who, as a Regent of the University, nearly wrecked the whole enterprise does not occur at all and, to come nearer home, no clue is given as to who at Scripps opposed Revelle's appointment as Director.

To be fair it must be said that Mrs Shor could hardly have told an unvarnished tale. She was asked by the Director to write the book and most of those mentioned in it are her friends. It would have been instructive to have reproduced some of the letters sent at that time to the President of the University and to see who said what and when; but it is clearly too much to expect.

After the turbulent reign and abdication of Revelle there was a short period during which Fred Spiess was Director. He picked up the pieces, smoothed the ruffled tempers and continued the building programme; he then handed over to Bill Nierenberg. Nierenberg had the difficult and unenviable task of maintaining and expanding Scripps in a time when the country had lost its belief in the worthwhileness of anything. For a

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while it looked as if Scripps would become 'Scripps rent-a-ship', an organisation that provided ships for anyone who could raise the money to run them. This has not happened; Scripps has remained what it was, an organisation practicing and encouraging all branches of oceanography; indeed it has taken on new responsibilities, such as the Deep-Sea Drilling Programme, and has built fine new buildings. Again the drama and the struggle are largely missing from the book. Again it all happens so smoothly and easily with no sense that, but for Nierenberg, it might have come out very differently.

In reading the book one does learn a lot about what happened and about who did what and went where. There are accounts of projects and expeditions with well chosen extracts from letters, ships' logs and radio signals. The facts are set out with great care and detail. It is clearly a book that everyone interested in oceanography and in Scripps should read. It is not the ultimate book on the personalities and the period, but it is an essential contribution to the writing of such a book in 50 years time. □

Sir Edward Bullard is Emeritus Professor at the Scripps Institution of Oceanography, La Jolla, California.

Predator-prey interaction in arthropods

A. F. G. Dixon

The Dynamics of Arthropod Predator-Prey Systems. By M. P. Hassell. Pp. 237. (Princeton University Press: Princeton, New Jersey, and Guildford, UK, 1978.) Hardback \$20; £10.70; paperback \$8.75; £4.40.

DR HASSELL has given us a lucid, clearly illustrated and eminently readable account of the development of analytical models of parasite-host interactions. This book is a most welcome exposition and summary of the current position in a rapidly developing aspect of ecology to which the author himself has been a major contributor.

Starting with the Nicholson and Bailey model the author develops step by step elegant analytical models framed in difference equations. This is achieved by the incorporation of more biologically significant parameters into the model. First, it is shown that stability can be predicted if an expression for density dependence is incorporated. A consideration of the functional responses of predators to prey density reveals the difficulties of extending relationships derived from studying mainly parasitoids. In true predators the search rate and handling time vary with stage of development of the predator and size of the prey, and this complexity poses difficulties in modelling this set of interactions. Consideration is given to the consequences of non-random search by a predator as opposed to the random search assumed by Nicholson and Bailey. More realistic prey and predator distributions tend to make the interaction very complex and difficult to explore analytically. However, the author shows that descriptive models have been useful in this context.

Next is revealed the powerful stabilising effect of mutual interference among searching parasites on parasite-host interactions, the constant area of discovery assumed by Nicholson and Bailey being a special case. However, although a widespread phenomenon in laboratory studies of parasite searching, it should be stressed that the extent to which mutual interference occurs in the field and the significance thereof still needs to be resolved. Many of the laboratory studies on mutual interference have been done at unrealistic densities. In considering the predator

rate of increase the author reveals another difference between predators and parasitoids. Prey density influences development, survival and fecundity in predators. If these factors are to be incorporated then the author suggests that we may have to abandon simple time dependent difference equations in favour of mathematically less tractable models.

Switching by predators from eating one prey species, to eating predominantly another species, as the proportion of the alternative prey increases, is shown to stabilise prey populations providing the predator population remains constant. Likewise, if switching is shown by the top predator in a community it can enable competing prey species to coexist. This is followed by a consideration of how it is possible for competing predators and hyperparasitoids to coexist.

The book ends with a most useful chapter on the theoretical basis for biological control which builds on the previous chapters. This reveals that an ideal parasitoid for use in biological control programme, all other things being equal, is one that has a high search rate and a marked ability to aggregate in areas of high host density; and it is argued that these are more likely to be attributes of specific rather than generalist parasitoids. However,

the case of the biological control of winter moth in Canada is used to show that when 'things are not equal' then predicting the outcome is difficult. Multiple introductions of parasitoids are shown now to be justified, as additional species will either coexist with the first species or replace it, and, whatever, the outcome, the equilibrium density of the host will fall.

The author justifies the book's title by regarding parasitoids as simple predators. He stresses that "true predator-prey models need to recover from the neglect at the hands of essentially parasitoid models". Only further study will reveal just how much of a special case the parasitoid-host interaction is and whether it can be usefully applied to the arthropod predator-prey system.

This book will undoubtedly be the standard text for both students and research workers in this field for some time to come. It is also a tribute to Nicholson and Bailey who, although unable to model the stability they observed in insect populations in the field, nevertheless produced a model which has been developed by the author and others to further our understanding greatly. At £4.40 the paperback version, is excellent value. □

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Creative personality

Macdonald Critchley

Chase, Chance and Creativity: The Lucky Art of Novelty. By J. H. Austin. Pp. 237. (Columbia University Press: New York, and Guildford, UK, 1978.) \$16.20.

NOT for a long time have I enjoyed a technical apologia so much. Disjointed it may be, but the book is none the worse for that. Obviously the author is not only a biochemist of conspicuous ability, but he is something much greater, a thinker who writes well, and who is versed in the humanities. The book opens in a naive, autobiographical but intensely interesting fashion. We learn how as a junior hospital officer his interest was captured by a fascinating and argumentative case-conference between two of America's outstanding neurologists. His stint in the pathological laboratory was highlighted by the opportunity to study that rare and deadly disorder, *metachromatic leuco-*

dystrophy. He became intrigued by the presence of highly coloured urinary deposits which in an odd fashion changed in hue from reddish-purple to a vivid golden brown. What was their nature and what was the meaning of their metamorphosis? Patiently over the ensuing years Dr Austin pursued his quest, following one clue after another, communicating with other workers far and wide. Eventually he narrowed down the responsible factor to a deficiency in the enzyme *sulphatase A*. This accomplishment led the author to investigate the nature of the Lafora deposits within the neurons in cases of *myoclonus epilepsy*. They proved to be comprised essentially of a glucose polymer. Important findings such as these inspired Dr Austin to extend his investigations into the chemistry of Alzheimer's disease and then the ageing of the brain.

This introduction leads on to sections 2 and 3, in which we are introduced to such fascinating topics as the nature of scientific discovery; the role played by mere lucky chance; the factor of intellectual preparedness; and the place occupied in research by serendipity. Concerning the latter, Dr Austin submits an allied but deviant conception which he calls "altamirage". This term was named after the "discovery"

of the prehistoric art within the caves at Altamira by Don Marcelino—more particularly by his dog, and especially by Maria, his 9-year-old daughter. It was this little girl who was the first to direct her gaze upwards, and thus become the discoverer of the glories of the "Sistine Chapel of prehistory". Altamira is defined as "the facility for encountering unexpected good luck as the result of highly individualised action". It transcends the boundaries of serendipity in its role of personal action in chance.

The erudite and stimulating chapters which make up the final sections are psychological and philosophical in nature, and deal with such subjects as, for example, the creative personality. In an interesting fashion the author reviews the considerable literature concerning the anatomy of creativity. He emphasises, in a manner which merits the attention of all senior academics who dispense power and patronage, Dragstedt's essay "Who Killed Cock Robin" in which is described the gradual death of creativity in a researcher who becomes enmeshed in too many committees. In Dragstedt's words, "the early days in the laboratory are the golden years for many scientists. After he becomes known, the volume of mail, telephone calls, number of visitors, organisational activities, including committees by the dozen, and demands for lectures, reviews and community activities grow insidiously and will destroy the creativity of the scientist if unopposed".

Throughout his book Dr Austin recurrently mentions the part which the arts may have contributed to his scientific career. Music is there, and more particularly an almost obsessive, lifelong preoccupation with colour. It is tempting to believe that the author is, or was, a synaesthetic thinker, though he nowhere explicitly says so. Likewise he speaks of a reality familiar to some who have closely studied the phenomena of the body-image, and which the reviewer has never before seen expressed in print. According to Austin, "Even now, writing these lines, I retain a clear visual image of the scene at the desk where these ideas burst forth. This time, in a kind of mental double vision, I am both a participant in the process and a spectator hovering over a point about five feet up and ten feet directly to the rear." (My italics.)

The reviewer has nothing but admiration for Chase, *Chance and Creativity*, and confesses that Dr Austin is a man he would very much like to congratulate in person. □

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Major flowering plant families

Peter D. Moore

Flowering Plants of the World. Edited by V. H. Heywood. Pp. 335. (Oxford University Press: Oxford and London, 1978.) £7.95.

It is an unusual experience to come across a book which demands unreserved praise; this one does. In both concept and execution this work is eminently laudable. It aims at providing a simple introduction to the major flowering plant families of the world; over 300 are described, so it approaches a comprehensive coverage.

The book commences with a brief introduction to the structure and biology of the angiosperms together with an outline of their evolutionary history and the criteria which are used in their classification. As the book is likely to attract a wide market outside the circles of academic botany, this is a very necessary section. Even within such circles it will prove a useful summary for students. It includes Stebbins' diagrammatic portrayal of evolutionary relationships within the flowering plants.

There follows a synopsis of classification in which families are arranged into their ten Orders. On the whole the Stebbins system is used, except where some small families are fused with closely related ones.

Technical terminology in plant morphology is, according to Heywood, one of the major obstacles for the amateur, which keeps him from the appreciation of much taxonomic botany. Undoubtedly he is right in this, and the provision of an illustrated glossary should go a long way towards demolishing this obstacle, especially as the attractiveness and clarity of the illustrations make it quite the most explicit glossary of its kind currently available.

The bulk of the text (300 pages) consists of a systematic treatment of the plant families. The description of each family includes an assessment of its distribution, both verbally and cartographically. The distribution maps are a unique feature of the book, which will prove particularly interesting to those interested in phytogeography. The verbal descriptions are necessary supplements; for example, although the family Magnoliaceae is

found in southeast Asia and in North, Central and South America, 80% of the genera come from the former region. A map on its own could be misleading.

The diagnostic features of each family are then described, and here the use of technical terms for the sake of brevity will necessitate frequent use of the glossary. Major subdivisions within the family are then described and finally its economic value is considered. The latter is a source of much interesting information and includes medicinal, timber and horticultural uses.

Quite the most striking features of the book are the illustrations. Each family has a few characteristic members illustrated both by habit drawings and flower sections. Many of the illustrations are reproduced in a sepia monotone with certain parts, often the flower or fruit, picked out in full colour. It is impossible to convey in words how aesthetically pleasing and technically informative this form of representation has proved. It combines the clarity of a line diagram with the naturalness of a well-composed painting. Victoria Goaman, Judith Dunkley and Christabel King, together with the publishers, must be congratulated on a set of illustrations which are unsurpassed in current popular botanical literature.

It would be petty to dwell upon errors in the text; they are present, but fortunately scarce. For example, the primrose is referred to as *Primula veris* rather than *P. vulgaris*.

There are no comparable books which could compete with this one, either in quality of production or in price (at under £8 it must be rated a bargain). Older books, such as G. H. W. Lawrence's *Taxonomy of Vascular Plants* (Macmillan, New York, 1951), which have served generations of students, lack the fine illustrations and the distribution maps of Heywood's book. These features will also appeal to the very considerable amateur market in both Europe and North America.

This book is more than a textbook of taxonomy, it is a publication which will stimulate an interest in plants within any who open its covers. It is an item which all biologists should have on their bookshelves. Oxford University Press are to be congratulated on the production of a book which will be of great service to botany and which is a credit to British publishing. □

Peter D. Moore is Senior Lecturer in Plant Sciences at King's College, University of London, UK.

Occasions of experience

Eric Ashby

The Star Thrower. By L. Eiseley. Introduction by W. H. Auden. Pp. 319. (Wildwood House: London, 1979.) £5.95.

THIS book is about Nature but it is an unusual book to be reviewed in *Nature*. Its author was a professor of anthropology but the book would score zero in an examination paper in anthropology. It is about Man in the Darwinian scheme of evolution but it is concerned with "the phase beyond the evolutionist's meager concentration upon survival".

Loren Eiseley was a poet who became a professional scientist. The essays and poems in this volume were collected for publication shortly before his death in 1977. They are autobiographical musings written by a deeply sensitive man who never reconciled himself with what he calls the "crystalline and icy objectivity" of the scientific attitude towards Nature. The imagery is vivid and personal; the style ornate and in places opaque. His naturalist's eye observes something—a playful fox cub too young to know fear, a spider keeping its web under surveillance, a fossil bone, a flint arrow head—these ignite his imagination and illuminate Man's relationship with the other living things around him.

The most interesting essays are those on science and humanism. Eiseley is distressed by reductionism. He does not dispute the need to think about living things at the level of atoms and molecules but he asserts that the things worth understanding about Nature cannot be discovered at this level; which of course raises the question: What, then, does he think is worth understanding? He answers this question by drawing the reader away from Darwin to Emerson and Thoreau. It is the numinous in Nature which matters to Eiseley, the attitude which the Germans call *Ehrfurcht*, the eye that sees the whole. "The finest intellect", he writes, "is that which employs an invisible web of gossamer running into the past as well as across the minds of living men and which constantly responds to the vibrations transmitted through these tenuous lines of sympathy".

Eiseley is not the only scientist who recognises the limits of reductionism and the dilemma of a theory of evolution from which the idea of purpose has been excluded. But, for my part, I do not think an appeal to the heady

prose of Emerson or the (sometimes querulous) protestations of Thoreau is likely to clarify these issues. Two essays on Thoreau—by far the best in the book—raise an interesting question, namely: What is the difference between imagination as it is used in science and imagination as it is used in poetry? Unfortunately Eiseley misses the opportunity to discuss this question. Imagination is as important in science as it is in poetry. The difference in the use made of imagination is this: the scientist disciplines imagination to make it comply with observation or experiment; the poet gives imagination free rein to range over whatever imagery his mind can design. Both these uses of imagination are legitimate. But it is confusing to mix the usage, and that—so it seems to me—is what Eiseley frequently does.

The Star Thrower has earned eulogies from other reviewers. I confess that I find the book disappointing. I think the reason for my disappointment is the use Eiseley makes of imagination. He uses it as a poet does, displacing (as he says of Emerson) "the sedate white doorstone into nature by something wild and moon-haunted, whether in science or art." In art, yes; Wordsworth, Emerson, Walt Whitman have

put on record vividly the human response to Nature. But in science, moon-haunted descriptions are not satisfactory records of Nature. Eiseley has tried to do something very important: to describe the intricacies of the biosphere as compellingly as Watson and Crick describe the intricacies of the double helix. It is an immensely difficult task and to have tried and failed is in itself creditable. Some poets—Robert Bridges in the *Testament of Beauty*, for instance—have got closer to it. So have some essayists—Lewis Thomas, for instance, in his charmingly allusive *Lives of a Cell*. I think the prize goes to A. N. Whitehead (and Eiseley generously acknowledges this), for he set out, with impeccable logic, the concept of process in evolution in a way which does enable one to think about the biosphere with the clarity Eiseley was seeking. It was Whitehead who linked together what he called "occasions of experience" like bars of music in a sonata. Eiseley's book, too, is a record of occasions of experience. It is not a clear record, but it is the testimony of a sincere and sensitive poet. □

Eric Ashby is a Fellow of Clare College, Cambridge, and Vice-Chancellor of Queen's University, Belfast.

Belief in a created Universe

P. E. Hodgson

The Road of Science and the Ways to God. By S. L. Jaki. Pp. 478. (Chicago University Press: Chicago; Scottish Academic Press: Edinburgh, 1978.) £9.50.

PROFESSOR JAKI is already well known as an historian of astronomy, with major works on the Milky Way, Olbers' Paradox and the Planetary System. In his *Relevance of Physics* he reviewed the history of physics and its relationship to other disciplines, and in *Science and Creation* (for review, see *Nature* 251, 747, 1974) he showed why science had its only viable birth in seventeenth-century Europe and not in the civilisations of antiquity. In the present volume, containing his Gifford Lectures for 1975 and 1976, he extends this line of thought by showing the deep connection between scientific creativity and natural theology. His thesis is that the belief in a created Universe implies the

epistemology that underlies scientific creativity, so that the road of scientific development and the ways leading the mind from the created world to a knowledge of the Creator are mutually sustaining and inextricably intertwined.

Science first came to maturity when Newton found the middle way between the empiricism of Bacon and the rationalism of Descartes. Newton's mind was dominated by a vision of truth incarnate in nature, a truth that can be revealed by a dynamic interaction between bold speculation and meticulous experimentation. His theory of gravitation was a creation of the mind—a leap beyond sensory data—but because it was a vision rooted in the data provided by nature it could become a vigorous science. Newton's awareness of the created nature of the world implied a recognition of its contingency, and hence the necessity of painstaking experimental work to uncover its secrets. The middle way found by Newton thus transcended empiricism without becoming trapped in a *priorism*.

The reverse side of this thesis is illustrated by detailed discussions of subsequent philosophers including Hume, Locke, Kant, Fichte, Hegel, Schelling, Mill, Comte and Mach. Their admiration of Newton's achievement and their ambitions to extend his

method far beyond physics was seldom matched by any comparable understanding of his scientific work, still less of its epistemological roots. Insofar as they departed from the epistemology inherent in the ways to God, they developed theories of science that would have been lethal if scientists had taken any notice of them. Fortunately they did not, so no great harm was done, at least to science itself.

The second great period of scientific creativity was initiated by Einstein and Planck, and here again the wellsprings may be traced to the same theistic source. Although initially Einstein was influenced by Mach, his scientific creativity gradually forced him to repudiate Mach's sensationalism. Its ravages are still evident in the philosophy of quantum mechanics and in Bohr's theory of complementarity.

Fragmentation of culture

W. H. Brock

Nature and the Victorian Imagination. Edited by U. C. Knoepfelmacher and G. B. Tennyson. Pp. 519. (University of California Press: Berkeley, Los Angeles and London, 1978.) £15.

"WHAT a glorious title, *Nature*, a veritable stroke of genius to have hit upon", wrote the mathematician J. J. Sylvester enthusiastically to *Nature's* editor, the astronomer J. N. Lockyer, on the first appearance of the journal in November, 1869. Yet, although the title was copied by French, Italian, Dutch and Norwegian commercial science publications, it is significant that when American scientists founded a similar journal in 1883 they chose the title *Science*. Again, T. H. Huxley, in translating a German nature poem for the first issue of *Nature*, could think of no more fitting a preface for a periodical "which aims to mirror the progress of that fashioning by nature of a picture of herself, in the mind of man, which we call the progress of science". Yet, as the editors of this elegant book point out, by 1918 any serious writer, painter or scientist would have thought it "impossible, quaint, archaic, Victorian . . . to rise from a bird's plumage to a consideration of man's position in the Universe".

That the scientific community pioneered this "denaturing" process few would deny. Put briefly, the collective theme of this interdisciplinary feast of a book is what S. F. Cannon has called "the fragmentation of culture", whereby the common

At the present time, increasing evidence of the singularity of the Universe is again pointing to its radical contingency.

Professor Jaki develops his theme with a masterly command of the original sources, and the text is supported by over a hundred pages of notes and references. It is so densely packed with ideas that it is not easy to read, but scientists, philosophers and theologians will find it a mine of stimulating analysis. Inevitably his thesis raises many other questions such as the relationships between the Christian churches and science, which have not always been harmonious, and the acceptance of science by radically different cultures; and these are apt future subjects for Jaki's pen. □

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religious, moral, poetic and scientific perspective of the early Victorians was gradually fragmented into, on the one hand, a literary consciousness which continued for a long time to seek symbols in Nature; and on the other, the various scientific forms of imagination which pursued separate, and sometimes, unrelated, disciplinary goals by measurement, experiment and abstraction. That "Ruskin . . . in looking at botanical specimens . . . saw nothing of what Darwin saw in them", is, for example, brought out by F. Kirchhoff in a fascinating analysis of Ruskin's floral mythology in *Proserpina*.

Apart from R. Smith's fine essay on the human significance of biology, the question whether value systems were completely abandoned by scientists is, unfortunately, little explored by the 26 contributors to this volume, which is primarily addressed to the literary reader. Concentrating on the changing artistic interpretation of Nature, there are only three essays by historians of science who actually explore the directions taken by chemists, physicists and biologists during the century. These, together with a fine essay on science education from the Arnold scholar, R. H. Super, stand for the systems of knowledge that challenged traditional "inscape". Ignored, except for the editors' allusion to the nature writer, W. H. Hudson, is the powerful tradition of natural history (whose social history has been so ably analysed by D. E. Allen in *The Naturalist in Britain* (Allen Lane: London, 1976) which may well be seen as continuing a moralistic science long after Darwin. By the same token, the essays are exclusively concerned with the intellectuals' imagination; whereas cursory reading of "working-class" natural history journals suggests that their readers continued, like the literary

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and artistic intelligentsia (but unlike laboratory-bound biologists) to find mystery and feeling in Nature.

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W. H. Brock is Director of the Victorian Studies Centre and Reader in the History of Science at the University of Leicester, UK.

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Empirical, useful, predictive enterprise

J. Z. Young

Reliable Knowledge: An Exploration of the Grounds for Belief in Science. By J. Ziman. Pp. 197. (Cambridge University Press: Cambridge, 1978.) £7.95.

PROFESSOR ZIMAN is a mathematical physicist but in this book he has devoted himself to the task of trying to define the aims and methods of all science. Wisely he begins with his own subject and bravely he goes on to extend his thoughts to biology and even sociology. Probably each scientist will like best the parts of the book that deal with subjects other than his own. But this is not to say that any of it is superficial or inaccurate, at least so far as my limited knowledge goes. Quite the contrary, he has succeeded in distilling the essence both of the results and limitations of very wide areas of science.

His general thesis is that "*the goal of science is a consensus of rational opinion over the widest possible field*" (p3) (italics throughout are his own). This is achieved because of the very specific "internal sociology" of the scientific communities, which allows for the communication of what he calls "consensual knowledge". The definition of this is that it is unambiguous and agreed to be true by all participants. The ideal form of it is mathematics and Professor Ziman takes a lot of trouble to explain how this approaches as ideal language. It is refreshing to find that he is under no illusions that it is perfect: "Unfortunately deficiencies of consensibility can be found even in the most sophisticated mathematical systems". In particular he emphasises that "the hope of finding a unique and *perfectly* logical language of this kind is vain". In order for scientists to agree, they have to share certain fundamental beliefs, for example, in the Kantian categories of space and substance. He is unwilling at first to define and delimit the range of these "supreme principles". As his exposition proceeds it becomes clear that he believes that they depend upon certain innate human capacities, especially "the mysterious skill that we call pattern recognition". Moreover,

this is not amenable to complete logical analysis and is not even completely uniform amongst all men.

Having thus warned us that mathematics is not a perfect tool he nevertheless believes it to be the best available and proceeds to give an excellent account of how it functions. His beautiful description of the diverse powers of synthetic and analytical methods is presumably elementary for the mathematician but simple and direct for the rest of us; and done almost without abstract symbols. He follows it through to discussion of the place of experiment and of models and hypotheses. Popper's criterion of falsifiability he holds to be "strategically sound but tactically indefensible", a polite way of saying that it is no use. Here, as very often, he speaks of the scientist actually at work, a rare virtue in a theoretician. Every theory can be falsified by some of the observations, the question is whether it is the best available; and he illustrates this by the controversy over the missing solar neutrinos, which are much less numerous than theory predicts. He rightly contrasts the "calm order and perfection of well established theories" with the "controversy, conjecture, contradiction and confusion" of the frontline of research.

His determination to show science as it really is leads him next to discuss the principles of observation and particularly pattern recognition. "The assumption that *all observers are equivalent*" is not merely a basic principle of Einstein's theory of special relativity; it is the foundation stone of all science". However, obvious though this seems, it in fact implies that "scientific knowledge cannot be justified or validated by logic alone". As observers differ, "statements about the real world are always subject to uncertainty. They cannot all be given precise status—'true' or 'false'—their logic is *three-valued*, falling into the categories 'true', 'false' and 'undecided'." Ziman gives much discussion of the implications of this thesis which, as he says, may not be of great importance in physical science but imposes limits in biology and especially in sociology. In particular he emphasises the great influence of the sociology of belief in determining the observations that are made. Every scientist must use the paradigms of his age and Ziman gives examples of how they determine the observations he makes and the interpretations that he puts upon them.

So much cautious scepticism might well lead to scientific pessimism. But Ziman is much too practical to be a defeatist. Whatever the fundamental

logical difficulties of validating science, "there is nearly universal human consensus concerning certain aspects of of the material domain". He leaves open the question "whether the categorical framework of everyday realism is the only possible scheme for describing and understanding nature in its physical aspects". He is content to leave it like this, but is not optimistic that "psychology and neurophysiology can arrive at a better understanding than, say, the ancient art of introspection".

Here I should disagree with him. Although he has taken the trouble to study these biological subjects seriously, I do not think he appreciates the speed with which they are approaching the capacity to achieve consensual communication that is useful even if not completely unambiguous. It is not too wild a statement to say that we are within sight of a 'complete' science of biology with principles that cover the origin, evolution and present operations of living things. It is true that the extension of this to include Man raises special difficulties. He rightly dwells in

his concluding chapters on the difficulties of precise definition of such concepts as "intelligence", "caste", "peasant" or "psychosis". In fact "we do not have a taxonomic framework for human behaviour, individual or social, in which the categories are both meaningful and distinct". True enough, and we all deplore the weakness of sociology. The difficulties introduced by hidden variables will of course always make the study of complex systems more difficult than that of simple ones. If the aim was to find some logically perfect science of man then indeed I should be pessimistic. But Ziman has taken pains to show us that the whole of science is simply an empirical, useful, predictive enterprise, characteristic of modern *Homo sapiens*. The life sciences are a part of this enterprise that is at present developing very rapidly, and I believe that his careful attention to them has already been very helpful and will be even more so in the future. □

J. Z. Young is Emeritus Professor of Anatomy at University College, London, UK.

Systematic theory of meaning

George Steiner

Truth and Other Enigmas. By M. Dummett. Pp. 470. (Duckworth: London, 1978.) £18.

FOR Michael Dummett, Frege's *Grundlagen der Arithmetik* is not only "unquestionably the most brilliant sustained performance of its length in the entire history of philosophy," but the whole of logic "came of age only with the work of Frege." It follows that the "most urgent task that philosophers are now called upon to carry out is to devise what I have called a 'systematic theory of meaning'." This systematic theory will proceed from the cardinal distinction between 'sense' and 'reference' in Frege and from Frege's concepts of the relationships between logic and mathematics on the one hand and logic and language on the other. Concomitantly, the philosopher, though fully aware of the attractive 'nihilism' in Wittgenstein's belief that philosophy is not "in the least concerned with establishing true propositions", will turn to Frege's systematic programme to establish a logical foundation for the determination of such propositions. And though, again, aware of the teasing finesse of Quine's thesis on the

indeterminacy of natural language propositions, he will reject this thesis. Only in this way, argues Professor Dummett, can philosophy aspire to a 'scientific' status and emerge from a 'scandalous' historical situation in which interminable effort and ingenuity "appears to engender nothing but unending disagreements."

Dummett's own papers represent this programme. They are closely-argued, rigorous and subtle steps towards a 'theory of meaning' capable of generating and testing a better understanding of 'truth', of the conditions under which a sentence may be correctly asserted, about whether the reality that renders key classes of empirical statements true or false can be shown to be fully determinate. These concerns are closely linked to a series of highly technical considerations (of special interest to the mathematician, to the mathematical logician but also, at several points, to the scientist in general) on the conflict between realists and intuitionists in mathematics, on the philosophical reach of Gödel's theorem and on the justification of deduction. Physicists, familiar with notions of time-reversal, will find much to instruct and delight them in two essays on "Can an Effect Precede Its Cause?" and "Bringing About the Past". Professor Dummett is a master of logic and of analytical exposition, and where one fails to agree—has he allowed for the characteristic and shrewd pragmatism which underlies Quine's thesis on the indeterminacy of translations between languages, a thesis which cannot be

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simply reduced to an intralinguistic model?—one does so at one's peril.

Professor Dummett's conviction that "we have now reached a situation" where what he postulates to be the fundamental philosophic problem, the theory of meaning, "can be resolved to the satisfaction of everyone" and, above all, be successfully resolved "within a finite time", is one of radical innocence. It may have at its core a certain personal element—even the most abstract, analytical thought has a living presence and agent behind it. There are tantalising hints that even in respect of mathematics, Dummett's provisional realism has a theistic basis or that 'metaphysics' is for him far from being nonsense if a systematic theory of meaning underlies it. Indeed, says, Dummett, "If I have made any worthwhile contribution to philosophy, I think it must lie in having raised this issue in these terms." The implications are very wide and of the greatest importance.

It is the more regrettable that the two papers in which Dummett deals with the history of 'linguistic philosophies' in this country, and with the

attacks (however technically inept) made on the narrowness, on the purely technical scholasticism of these philosophies, should decline to engage the real issue. This issue is not whether Strawson has been misinterpreted or whether Ayer has seen the importance of Frege's logic. It is whether or not the concerns which Professor Dummett so brilliantly pursues are central, are adequate to the concept of philosophy in society and the lives of individuals. The charge, and Dummett does not answer it, is simply this: Oxbridge linguistic logic has trivialised the enterprise of the philosopher. It has sought to eliminate ethics, aesthetics, metaphysics in the very sense in which Dummett invokes it. It has chosen to ignore, in Owen Chadwick's remarkable phrase, the extent to which, since the 1930s, "the agonies and passions of mankind started to enter and complicate the philosophies." The challenge is a real one. No-one would meet it better than Professor Dummett himself. □

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Panorama of modern neurobiology

Leslie Iversen

Molecular Neurobiology of the Mammalian Brain. By P. L. McGeer, Sir John C. Eccles and E. G. McGeer. Pp. 644. (Plenum: New York and London, 1978.)

THIS substantial volume represents the results of an ambitious undertaking by two biochemists who have pioneered the application of biochemical approaches to studies of chemical neurotransmitters in the brain, and one of the most eminent neurophysiologists of our time. An ambitious undertaking because they seek to bring together in one volume virtually all of the achievements of modern cellular neurobiology and to show how far this basic information can be used to explain the integrative functions of the mammalian brain. This is promising stuff, and the reader is further encouraged by the authors' principle that they should provide not merely a bare factual account of present knowledge, but attempt to give some outline of the key events in the historical development of such knowledge from the interplay of hypothesis and experiment. We may even hope that the basic cellular data will

throw some light on the workings of the human brain—"This organ, weighing about 1.5 kg, is the most highly organised matter known in the universe".

The *Molecular* title indicates a leaning to the 'wet' approach to neuroscience, which emphasises the explanation of brain function in chemical terms; but this is in fact misleading, as virtually all aspects of cellular neuroscience are dealt with in the first and largest two sections of the book. These, in some 360 pages, review basic information on the architecture, organisation and signalling properties of neurones, and concepts of chemical synaptic transmission in the periphery

and in the central nervous system (CNS). A series of fairly detailed chapters summarise current knowledge of the specialised biochemical properties associated with neurones using acetylcholine, amino acids, catecholamines, or 5-hydroxytryptamine as transmitter, and others cover the new field of 'promising peptides' and various other putative CNS transmitters, accompanied in each case by a brief outline of the actions of drugs on neurotransmitter mechanisms.

The coverage of neurotransmitters is good and up to date, although more explanation and examples could have been given of ligand-binding techniques for the study of neurotransmitter receptors in CNS, a development which has had a major impact on neurochemical studies of such mechanisms in recent years. The undue emphasis given to acetylcholine and catecholamines (almost 100 pages) continues to reflect the inadequacy of our current knowledge of other CNS transmitters, rather than the true importance of the monoamines in CNS function.

A third section of some 200 pages gives a wide-ranging review of integrative aspects of brain function, from development and plasticity to motor control mechanisms, learning and memory, and perception, speech and consciousness. The aim is to refer back in this section to the basic properties of neurones described previously. This aim is sometimes successfully achieved: for example, in the description of the extrapyramidal motor control systems in basal ganglia, in which anatomical, physiological and neurotransmitter aspects are well integrated, with discussion of human basal ganglia disease, such as Parkinsonism and Huntington's Chorea and their treatment by drugs, neatly interspersed. On the other hand, the description of cerebellar function completely omits any mention of the neurotransmitters involved, despite reference in earlier sections to the important role of the putative amino acid transmitters, γ -aminobutyric acid and glutamate, in this region of the brain. An ambitious chapter on basic aspects of behaviour includes a description of central endocrine influences and the CNS actions of psychoactive drugs, together with a review of current neuropharmacological theories of mental illness. As complexity increases with each succeeding chapter it is inevitable that the book ends with a discussion of perception and consciousness, in which the dualist-interactionist hypothesis—described in more detail in the recent book by K. R. Popper and J. C. Eccles *The Self and Its Brain* (Springer: Berlin, 1977; for review, see *Nature*, 272, 770, 1978)—is restated. The reader is given plenty of advance warning of this conclusion: in discussing the ques-

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tion of consciousness (p3) "We shall see that it is beyond our comprehension how these subtle properties of the conscious self came to be associated with a material structure, the human brain, that owes its origin to the biological process of evolution." However, whether one agrees or not with this strong dualist stance, we could all agree that "We believe it to be important for the molecular neurobiologist to be confronted with the problems arising in higher brain functions."

In any volume with as wide a scope as this one, each of us will no doubt find what seem to be important omissions or failures of emphasis. For example, I found it hard to understand why so little attention was given to a clear description of modern anatomical tracing techniques, using anterograde and retrograde flow of marker materials, in view of the enormous and continuing impact such techniques are having on our understanding of neuronal circuits in mammalian CNS. (Pick up any current issue of a brain research journal and count the number of papers involving peroxidase tracing methods!)

The overall impression left by this book is that of a courageous enterprise which did not quite succeed in its objectives. The variation in styles between the authors is often very obvious, and this together with the attempt to cover almost *all* aspects of mammalian neurobiology inevitably makes the book somewhat disjointed, with the individual chapters often divided and sub-divided over again into a multitude of smaller sub-sections. The illustrations, however, are copious and of excellent quality, and there is a substantial bibliography, with more than 1000 references (though with few more recent than 1976). One is left with admiration for the energies of the authors who have succeeded in putting together an attractive volume, but also in some doubt as to its likely readership. The book is probably too large and expensive to become widely used as a neurobiology student text, where other excellent volumes in any case already exist—for example, S. W. Kuffler and J. G. Nicholls' *From Neuron to Brain* (Sinauer Associates: Sunderland, Massachusetts; UK Publisher, Place, 1976). Nevertheless, it deserves to find a wide readership, and will certainly provide an accurate and sometimes inspiring panorama of modern neurobiology for those who want to know more about the field as a whole, or who feel the need to bring up to date what they thought they once knew. □

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Boundaries of palaeo-anthropology

G. E. Kennedy

Early Hominids of Africa. Edited by C. Jolly. Pp. 598. (Duckworth: London, 1978.) £28.

HISTORICALLY ill-defined, the boundaries of palaeoanthropology have recently been delineated by several excellent volumes. Although all of these volumes have emphasised the interdisciplinary character of palaeoanthropology, none has so effectively designated the scope and nature of the contributions from geology, anatomy, biology and archaeology better than this one.

Early Hominids of Africa is comprised of papers given at a conference held in New York more than five years ago. That the conference was organised (largely by Cliff Jolly, who also edited the volume) with care and some foresight is attested to by the overall currency and freshness of the papers. A number of the papers are, in fact, of considerable value in designating the current structure of the discipline.

Glynn Isaac, in his contribution, for example, discusses the scope of palaeolithic archaeology within the context of palaeoanthropological investigations. He points out that such studies focus less on traditional questions of tool typology and more on broader areas of tool function and the behaviour, selection and origin of raw materials. His suggestion that tools represent "self-recording behaviours" typifies the approach where more detailed and subtle questions are now asked of empirical data than was usual in earlier years.

So, too, with the palaeoecological papers contributed by Butzer, Behrensmeyer and Cooke. These papers, each from a different research perspective, focus on the reconstruction of the environmental context of the early hominids. Butzer's paper is perhaps the most successful of these and indicates the new directions taken by the various contributory disciplines within the context of palaeoecological reconstructions. Butzer argues that many of the environmentally deterministic models of anthropoid and hominid evolution were erroneously based on unidimensional reconstructions and both the

environment and the sources of selective pressures were more complex than the earlier, more simplistic models would allow.

The section on anatomical evidence, the largest in the book, reflects a strong trend in research interests away from the cranium; all but two of the papers deal with analyses of postcranial anatomy and locomotor capabilities. Papers by Day, Robinson and Lovejoy are informative in these areas, although they provide little that has not been published elsewhere. The two papers on cranial morphology are innovative and highly specialised. Wallace's study on the ramifications of early closure of the premaxillary suture in hominids is both insightful and soundly based on the fossil evidence. His discussion on the ontogenetic, phylogenetic and adaptive aspects of this process is an excellent example of the subtlety of the questions now being asked of the fossil data. So, too, with Holloway's discussion of endocranial casts which continues his series of stimulating, frank and challenging papers.

A section devoted to the interpretation of diversity in early hominid samples continues the interdisciplinary theme. Delson's paper on cladistic analysis, which emphasises trait congruity as a taxonomic tool, contrasts strongly with Campbell's, which seems to emphasise temporal congruity for the same purposes. Simons contributed an analysis of hominid evolution from a vertebrate palaeontologist's viewpoint. Although valuable for methodological as well as interpretative reasons, this paper is of particular interest for its discussion of *Ramapithecus freybergi*, a later Miocene hominid from a site near Athens.

Papers on the most productive of the east and south African localities were submitted by the sites' principal investigators. Although much of this information has been published elsewhere, these concise summaries are useful.

The long delay in publication has resulted in very few shifts in the interpretation of data. The most noticeable of these shifts involves Johanson's evaluation of hominid material recovered at Hadar, Ethiopia. In 1972, he suggested that two taxa were present at Hadar, a view recently formally rejected in favour of a single taxon. The only significant criticism which can be levelled at this volume is the cost. The high price will put it beyond the reach of most advanced undergraduate and graduate students, groups which would benefit most from it. □

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Technological medicine

Maurice Lessof

Beyond the Magic Bullet. By B. Dixon. Pp. 249. (George Allen and Unwin: London, 1978.) £5.50.

THE essential thesis of this readable and well written book is that expensive, technological medicine has failed. It has distorted our approach to health problems and has led to the neglect of the epidemiological, social and psychological components of illness. Worse than this, it is not even effective. Waterborne infections, which kill an estimated 10 million people annually throughout the world, could be contained more readily by measures that are concerned with nutrition, hygiene and water supplies than ever they could by antibiotics.

There is a second theme which is a little more difficult to sustain. The author believes that, in a world setting, the approach to disease through individual causes and specific cures has largely been a fiasco. 'Specific aetiology' is an idea that has had its day; for even infections with an identified cause are very complex in their origins. A technological approach to treatment is inappropriate in the Third World, where the death rate for measles is increased 400-fold by malnutrition.

The fallacy of this proposition is perhaps worth considering, for the argument is very seductive. The use of vaccines, much applauded by the author, is the fruit, *par excellence*, of research into 'specific aetiology'. It is also, incidentally, a product of the much-criticised pharmaceutical companies, with their 'magic bullet' mentality. In making the case for better nutrition and hygiene, it is tempting to deride the scientific approach. Yet vaccination has eradicated entire diseases from many countries throughout the world. The evidence of this book, so impressive in other ways, cannot therefore be accepted for this curiously science-bashing purpose.

The author's thesis is a powerful one and may be re-stated in another way. The Western approach to medicine is characterised by a search for antidotes and glamorous technology. It has led to overspecialisation, overprescribing, and the development of incongruously elaborate techniques for curative medicine and surgery. It fails to take into account the fact that the illnesses of mankind are determined by a combination of many factors, which require social remedies rather than pill-pre-

scribing and high-technology doctoring. In the West, cigarette-smoking and obesity, in the Third World nutrition and hygiene—these should receive our attention and our emphasis.

In its writing, this book is delightful, and the historical section includes an account of Pasteur's experiments which is strangely exciting. (Poor Jenner gets no credit, but never mind!) The author is not merely an anti-medical Illich, following a fashionable witch-hunt against doctors. He notes that some at least of the medical profession share his concern with the relationship between social problems and health care. The medical establishment is, however, unable to cope.

The problems are indeed formidable—how to assert, in a liberal democracy, an effective anti-smoking, anti-alcohol, anti-obesity policy; how to persuade a Third World country to modify, however slightly, the priority given to prestige hospitals and universities, in order to emphasise the importance of hygiene, health education and good water supplies; and how to encourage solutions which, even when they cost nothing, require a firm stand by politicians, who themselves depend on populist support.

Much of the quoted evidence is beyond dispute. There are some massively important problems, which we neglect at our peril and which require largely political solutions. The medical or scientific reader may find little comfort in the author's message. The implication, almost throughout, is that the purveyors of modern, conventional medicine and research are oblivious to the real world, shackled to an obsolete view of current health needs, obsessed by high technology and magic bullets, and unable or unwilling to adapt either their thinking or their teaching of the next generation of doctors. This is simply not true. The problems are as formidable as the author says they are. In seeking to combat them, it needs to be more clearly stated that doctors, and even scientists, are quite frequently on the side of the angels.

Switching from the Third World to the sophisticated West, there is not only a need to change our social priorities but also a need for more humanity in our provision of health care. This is illustrated by a vivid account of a patient with ankylosing spondylitis, who had an unquenchable determination to get better, though "one of the experts put the chances of recovery as one in 500; another said he had never witnessed recovery from this comprehensive state". (On the story given, this was probably not ankylosing spondylitis at all but post-infective reactive arthritis, from which a rapid recovery is usual. Even in true anky-

losing spondylitis, it should be stated that some 80–90% of patients are earning their own living 20 years later.) The story is a good one, recording a patient's determination and also a doctor's unstinting support for an unconventional treatment which had nothing to commend it except for the patient's faith. The point is well made. A practising doctor looks after sick people, not disembodied disease. Hope and encouragement may do more than medicine—even more so in diseases such as cancer, in which definitive medical treatment may not be available.

Having reviewed all the evidence, this book suggests that we must deploy both our doctors and our financial resources to better effect. The message could, however, be dangerously misread. On the advice of our politicians, we in Britain have already suffered an expensive and ineffective reorganisation of the Health Service; a wasteful but well-meaning allocation of money to ineffective types of social work; a crazy emphasis in our Community Health Councils, which give priorities to 'watchdog' functions rather than to a responsible collaboration with professional health workers; and an opportunistic encouragement of specialised kidney units which are now, for political reasons, deprived of funds.

What about the needs of the Third World? Who can doubt that great benefits could follow from better nutrition, hygiene, good water supplies, and the Chinese barefoot doctor's approach to health education and simple health care. But even here, the problem is not a simple one. In many parts of the world the mere fact of a reduced infant mortality rate results in the exposure of even more children to the ravages of malnutrition. In this context, social priorities must therefore include population control. This, the most important problem of our time, is merely touched upon in this book. Alone among the Third World countries, the Chinese have shown that population expansion can be controlled by means of public education and persuasion. In the West, however, this social and political approach has not succeeded. Here the contraceptive pill and the intra-uterine device have, indeed, provided another victory for high technology, and their future potential blessings have yet to be realised.

In summary, the siren song of the anti-science lobby needs to be heard but then resisted. The moral to be drawn from this book is absolutely clear. We need both social policies and high technology to solve our problems. It is a partnership, not a conflict. □

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Complex mental functions

Kevin Connolly

Mind in Society: The Development of Higher Psychological Processes. By L. S. Vygotsky. Edited by M. Cole, V. John-Steiner, S. Scribner and E. Souberman. Pp. 159. (Harvard University Press: Cambridge, Massachusetts, and London, 1978.) £8.75.

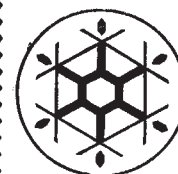
L. S. VYGOTSKY is known largely to the English reading community through a little book, *Thought and Language*, which has received very wide acclaim. It was published posthumously in 1934, suppressed in 1936, reissued in 1956 and translated into English only in 1962. The book has had a great impact on Western psychology and revealed Vygotsky as a truly original thinker and insightful observer of children. He was born in Orsha in Byelorussia in 1896 and graduated from the University of Moscow in 1917 having specialised in literature. From 1917 to 1923 he taught literature and psychology in a school and began his systematic work in psychology only in 1924. In 1934 at the age of 38 he died of tuberculosis. During this all too brief period of research in psychology he was immensely productive. He worked first at the Institute of Psychology in Moscow and then in the Institute of Defectology, of which he was a founder. Over this 10 year period he taught courses in Moscow and Leningrad, undertook a medical training, and wrote more than 160 books, papers, reviews and lectures. Much of this work remains unpublished and very little has been translated into English. It is now over 40 years since Vygotsky died and yet much of what he had to say is fresh and deeply significant for contemporary psychology. This newly-translated material is of historical interest but not primarily so; he was writing clearly about fundamental issues which Western psychology has recently been congratulating itself on having articulated.

Vygotsky was interested in complex mental functions, or as they were more usually called 'higher psychological processes'. He rejected the notion that an understanding of these processes could be gained from an extension of the principles derived from animal psychology, and like the Gestalt psychologists did not believe that an adequate analysis was to be found in

reductive stimulus-response theories. He sought a comprehensive approach which would provide both description and explanation in terms acceptable to natural science. His goals for such a programme were immensely ambitious including as they did the identification of relevant brain mechanisms, the developmental history of behaviour, and most important specifying the societal context in which the behaviour developed. That he did not fully achieve these goals is hardly surprising but he did show himself to be an astute and remarkable thinker.

For Vygotsky the developmental method is a central feature of psychological science. He saw the scientist's task as one of reconstructing the origin and course of the development of behaviour and consciousness. A distinctive theme of his writing is the emphasis which he placed upon the unique qualities of man. As a species we actively realise and change ourselves in the varied contexts of history and culture; in the course of his history man affects and changes nature to create for himself new conditions of existence. To the Western tradition Vygotsky seems less an experimenter and more a thinker. He was a brilliant observer of children and an ingenious experimenter, but his work is not characterised by elaborate experimental designs and statistical analyses. Rather the experiment is a means of gaining insights which illuminate the ideas and carry them along. The experimental 'leg work' will no doubt be done by others, who perhaps find thinking harder.

The book contains eight of Vygotsky's essays, the first five of which are taken largely from a hitherto unpublished manuscript of 1930 entitled, *Tool and Symbol in Child Development*. Here Vygotsky attempts to relate Marx's Theory of Society to concrete psychological questions. The concept of tool implies a specific human activity, the transforming effect man has upon nature in gaining mastery over it to serve his ends. Like tool systems, sign systems (speech, writing, number) are created by societies and change with the level of cultural development. In this way the mechanism of individual developmental change is rooted in society and culture. The last three essays explore educational implications of his ideas. Again the penetrating quality of his thought is striking. He explores the relationship between learning and development, and in his concept of the "zone of proximal development" can be seen the antecedents of the competence/performance distinction which psychologists have made much of over the



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past decade. He proposes too that intelligence could be better assessed by an examination of the child's capacity to benefit from teaching rather than the normative comparisons embodied in psychometric tests. In an essay on play he concludes that it is not a predominant feature of childhood but that it is a leading factor in development. The last essay on writing is a masterpiece which has profound practical implications in education.

This little book is an intellectual excitement; it abounds with all manner of ideas, insights and novel formulations. For anyone feeling jaded with contemporary cognitive or developmental psychology the book should lift the spirit; for the graduate student in search of ideas it cannot fail to raise and widen the horizons. The editors provide, two by two, a useful and sharply focused introduction and concluding piece.

Vygotsky comes high on my list of people I regret not having met and I am grateful to the editors for having helped me know him in the next best way. I recommend the book warmly and without reservation. □

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Invitations to enquiry

Mary Waring

Science, Curriculum, and Liberal Education: Selected Essays. By J. J. Schwab. Edited by I. Westbury and N. J. Wilkof. Pp. 394. (University of Chicago Press: Chicago and London, 1978.) \$24; £16.80.

FOR some twenty-five of his fifty years at the University of Chicago, Joseph Schwab participated in an experimental undergraduate programme in general education, and it was this experience that gave impulse and direction to all his writing. This book contains a selection of twelve of the fifty or so essays that make up the bulk of his published work, an Introduction written by the Editors, and a bibliography.

The Introduction outlines the context in which Schwab's ideas developed. It stresses the continual interplay between, on the one hand, reflection upon the immediate practical concerns of his day-to-day teaching and, on the other, extensive explorations in psychology, psychiatry, literature, philosophy, science and the philosophy of science, an interplay that was to become the hallmark of his approach to education. His mastery and use of many perspectives and this interrelating of ideas and action gave Schwab both an intellectual framework and an integral and distinctive approach to problems. Both were greatly enriched by continuing dialogue with colleagues like Louis Thurstone, Irving Lorge, Ralph Tyler, Robert M. Hutchins and, above all, Richard McKeon, who introduced Schwab to hermeneutically inspired interpretative schemata and, as a former student and colleague of John Dewey, gave him direct access to a philosophy that was to be a major influence on his thought.

Although many of the essays are well known and highly regarded, the fact that most have been written for special occasions and purposes and have hitherto been published individually in a variety of journals, has meant that they have proved difficult for many readers who have come to them in isolation or at relatively long intervals of time. Collecting them into one volume and providing the orientation afforded by context creates a whole which is much more than just the sum of its parts; it reveals so vividly the comprehensiveness and coherence of Schwab's understanding and it encourages and facilitates greater fluidity

of thought on the part of the reader. Grouped under three headings ("On Liberal Education and Science", "On the Foundations of the Curriculum" and "On Curriculum Building"), the essays are concerned primarily with a carefully reasoned enquiry into the practical problems of giving new meaning and vitality to the conception of a liberal and liberalising education and, within it, to the teaching of science. Schwab leads the reader through the complex web of considerations which enmeshes each of the issues explored—enquiry and interpretation, personhood and experience, scientific enquiry and the nature of science and liberal education, educational testing, teacher education and curriculum development and research—providing a series of deep insights and provoking reflection upon the articulation of ideas and action in the search for means and ends. There is no attempt, however, to argue for

outdated, elitist conceptions of liberal education: the concern is with the search for essentially practical ways of helping the young to become 'actively intelligent' people who enjoy and thus continue to seek out knowledge and understanding, and who possess the critical and organising powers that make possible informed choice and action.

This is a challenging and important book, offering none of the slogans and easy solutions of those whom Schwab describes as "honing a problem down until it looks simple" or basing their ideas on "unexamined notions of reality". Emphasising, always, that matters of the curriculum are matters for doubt and critical enquiry, Schwab is concerned to "move men to action", rather than to prove or to prescribe.

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A forbidden transition

Fred Dainton

Higher Education Developments: The Technological Universities, 1956-1976. By P. Venables. Pp. 407. (Faber and Faber: London, 1978.) £25.

THIS book has a simple structure. In the introductory chapter the reader is taken at a brisk trot through the history of post-school technical education by way of the Percy Report; the establishment in 1956 of the Colleges of Advanced Technology (CATs), and of the National Council for Technological Awards; the Robbins Committee's recommendations of 1963, most of which were accepted almost immediately by the Government and which led to the CATs being transformed into technological universities with a consequential transition from ownership and government by Local Authorities (or Government in the case of Loughborough); to self-government and financial support from the University Grants Committee (UGC). The next eight chapters are a description of all that happened in and to these institutions in the decade 1966-1976. This section is abundantly provided with graphs and tables and is a gold-mine of factual information which is made very easy to quarry by the use of a very well constructed index. The tenth chapter is entitled "The Polytechnics" and is in fact a fascinating comparison of these institutions with the technological universities, in which the author seems to

invite—almost quizzically—the reader to look at the facts assembled in four tables and draw his own conclusions. In the last two chapters the author looks at higher education in a broader perspective, as it is and as it is likely to be, and he also lifts the veil ever so discreetly on his own credo and ideals. The book ends with four appendices; a glossary of acronyms, potted biographies of the eleven institutional actors; a few more statistics which I feel would have been better placed in the text; and the questionnaires which evoked the responses on which much of the information purveyed by the book is based. The wealth of data assembled in the book contrasts somewhat strangely with a curious omission. Nowhere did I find a really clear definition of technology and how it should be distinguished from natural science and engineering—if it is.

The non-British reader may be surprised that a whole book should be devoted to the detailed examination of such a small subject—namely, the emergence, infancy and early childhood of eleven institutions containing only about one-tenth of the total university population and therefore only about half that proportion of the total in higher education. The significance of the subject does, however, go far beyond what this mere numerical comparison might suggest, for the process by which the technological universities were created can never be repeated for as long as the policy of the "binary system" holds sway. That transformation would now be a "forbidden transition". The book is therefore important as a case study of special value to two groups of people. On the one hand it will be important to those who will determine future tertiary education policy in Britain and on the other hand to the historians

of the politics of education. It can be strongly recommended to both because it is soundly based on a careful scrutiny and scholarly evaluation of documents and an exhaustive enquiry; above all, it is written by an "insider" (the author's own term). This last point is of special importance for although there are many pieces, published and unpublished, which are written about higher education in Britain few of the authors have Sir Peter Venables' very wide experience. These writers fall into a very wide range. Thus at one extreme the authors are lonely scholars, either MA or PhD thesis writers or their own senior colleagues, more often than not embedded in a School of Education, itself on the fringe rather than in the mainstream of the activities of its parent institution. Such authors' sources are restricted to public documents and their experience is unlikely to embrace that of treading the corridors of a local education authority, the Department of Education and Science, the Scottish Education Department, the Welsh Office or even the UGC. The fruits of their labours are, however, likely to be published perhaps in book form or as articles in journals for all to see, dissect and evaluate. At the other extreme there are the civil servants whose duties are to prepare influential position (and policy) papers which rarely ever find their way into the open literature—at least legitimately. Their own direct experience of higher education is likely to have been limited to a few years at a university ten, twenty or even thirty years earlier. So the scholar suffers from ignorance of the "real", as distinct from the "openly acknowledged", political and intellectual reasons for decisions; and the civil servant who is well placed to influence those decisions has no living, working contact with the "coal-face" of higher education.

This gap is deplorable, as it is likely to militate against the formulation of the most beneficial policies. How is it to be bridged? The Royal Commission is one device. It is certainly open and thorough but also often cumbrous and slow; and it is not a permanent bridge. There are a few helpful signs in, for example, what seems to be a greater willingness and/or freedom of the public servant to discuss issues with scholars and researchers and the welcome tendency to issue "Discussion Documents" and "Green Papers" before decisions are taken. Then there is the occasional forum or symposium which is, however, rarely productive of meaningful interaction between the civil servant who, restrained by the rules and conventions of his employment, may elect to stay silent at a public gathering on just those topics which, being of greatest contemporary interest and importance, are therefore also the most sensitive issue.

The situation would be much improved if there were some exchange of people between posts in Academe and the Public Service. But mobility, though repeatedly

advocated as desirable, remains and is likely to remain far too infrequent. In our British system the best we can hope for is the lucky chance either that academics and ex-civil servants are able to collaborate (as in the case of Becher, Embling and Kogan's brief study of higher education) or that some able person who has been deeply involved as a teacher and administrator in higher education institutions for most of his life, helping to shape them during a period of rapid development, who has also served on many national bodies in ways which have brought him into close contact with civil servants, local authority officials and politicians, will make time to take stock of the past and present and point out the options for the future.

Such a man is Sir Peter Venables. His career in higher education, especially in technological universities and their precursors, will be known to many as

qualifying him well for this task, to which he has also brought the scientist's insatiable appetite for facts and a method of enquiry borrowed from the woman who is both his sociology instructor and wife. The result is predictable and well worth the effort. I have only two regrets. The style is almost too 'dead pan', too detached and at a number of places I would have liked Sir Peter to be a little more 'unbuttoned' and to have given us the benefit of his own opinions even when they could not be completely buttressed by evidence. His own long and devoted service to the cause of technological education have earned him that right. My second regret is that the price should be so exorbitant and therefore the book may not get the circulation it deserves. □

Sir Frederick Dainton was, until 30 September 1978, Chairman of the University Grants Committee, London, UK.

Energy melting pot

Peter Rost

Energy and the Environment: Democratic Decision-Making. Edited by C. Lenzer, C. Phipps, J. Valleix and J. Surrey. Pp. 141. (Macmillan: London, 1978.) £5.95.

It was not until the oil crisis of 1973–74 that politicians woke up to realise that they now had to face yet another challenge. Until then the provision of energy at an acceptable price was taken for granted. Energy policy was regarded as the preserve of technical experts and the balance between supply and demand was satisfied by the market. Now energy has become a dominant world political problem.

The energy debate has not lacked documentation. Students anxious to be alarmed or reassured about the prospects for mankind's future energy resources have been deluged with reading material. But each such contribution to the argument has been a personal interpretation, or a consensus by an expert group. What has been less readily presented to the informed but enquiring reader, is an actual cross-section of the debate itself.

This is why this book is an important contribution to the evolving energy debate. It records a two-day colloquy organised in November 1977 by the Council of Europe, which brought together 70 politicians from Europe and North America, with an equal number of independent experts. The Parliamentarians represented a formid-

able array of spokesmen many of whom have established their national and international political reputations for their specialist contributions to energy studies, whereas the technical and industrial experts included many reputable figures from international organisations concerned with energy problems, of high standing in scientific, academic and international circles.

Such a rich mixture into a two-day melting pot could easily have created an indigestible meal. But the themes were well defined and the debate was disciplined. Four principle aspects were tackled—energy supply and demand to the year 2000; the contribution of fossil fuels and renewable sources; the nuclear debate; and options for the future.

Perhaps surprisingly, some consensus emerged. No one energy source will solve the future, real costs will rise and conservation must move up the league table of priority options.

Revealing too, is the admission by decision takers, that they (the decision takers) will have to face up to unpopular measures! Long term policies to provide enough energy, yet protect the environment, maintain living standards and preserve democracy, may require electorally unpopular choices.

This book should help to reassure the informed public that, although the experts will never agree any more than politicians, at least the debate is being carried forward on solid intelligent foundations of scientific knowledge and political maturity. We must hope that Parliamentarians will follow up with the correct policy decisions. □

Peter Rost is Member of Parliament for South-East Derbyshire, a Member of the Select Committee on Science and Technology Sub-Committee on Energy Resources, and Member of the Conservative Parliamentary Energy Group.

Civil nuclear policy

Walt Patterson

The Politics of Nuclear Power. By Dave Elliott, with Pat Coyne, Mike George and Roy Lewis. (Pluto Press: London, 1979.) Paperback £1.95.

THERE cannot be many issues which would bring John Biffen, MP, and Robin Cook, MP, into the same voting lobby at the House of Commons. Each is generally considered to be well toward the outer flank of his party—Biffen toward the right of the Conservatives and Cook toward the left of Labour. Yet on 15 May 1978 Biffen and Cook were together among the 80 Members who challenged the joint wisdom of both Front Benches and voted against the proposed expansion of nuclear fuel reprocessing at Windscale. Articulate Parliamentary critics of British civil nuclear policy, whose numbers have increased impressively in the past two years, speak from virtually every corner of the House—as do nuclear advocates.

It is an interesting phenomenon, worthy of much closer scrutiny. But it is not the subject of *The Politics of Nuclear Power*. Perhaps it should be, for the issues which crystallise around civil nuclear policy do not lend themselves easily to the conventional left-right of traditional party politics in Britain. However, Dave Elliott and his colleagues have here set themselves a more immediate task: that of persuading trade union members, and others on the left of the political spectrum, that it is in their interest to re-appraise official union policies which presently support civil nuclear expansion. Elliott and his colleagues can point to France, where the Confédération Française Démocratique du Travail (CFDT), the country's largest union, has played a leading role in attacking French government plans for headlong nuclear programmes; and to Australia, where the Australian Council of Trade Unions has been confronting the government for some four years, opposing official plans to mine and export uranium. However, it is also true that trade unions elsewhere—notably in the US and the Federal Republic of Germany—have been among the most enthusiastic and vehement proponents of civil nuclear activities. Even on a broader party-political basis the case is

by no means clear-cut. French and Japanese socialists, among others, are critical of nuclear power; but West German and Swedish Social Democratic governments have pressed ahead with nuclear expansion—only to find, in the Swedish case, a non-socialist party ousting the Social Democrats from office on a promise to phase out the nuclear programme. In the Soviet Union, of course, the civil nuclear programme has long had top priority, leading to the ironic spectacle late in 1978 of a party of American journalists, touring Soviet nuclear facilities under the auspices of the US Atomic Industrial Forum, champions of nuclear free enterprise. In short, the picture is confused, and confusing, and the time-honoured soapbox rhetoric of two-legs-good, four-legs-bad—or indeed *vice versa*—is unilluminating and unhelpful.

Fortunately, Elliott and his colleagues try to avoid sloganeering, concentrating on substantive factual information and analysis of particulars. An introduction by Elliott opens by declaring that "This book focuses on a number of problems, questions and issues relating to nuclear power likely to be of concern to workers in the nuclear industry and elsewhere". Elliott then offers a brief description of nuclear technology and its status in Britain and elsewhere. The main body of the book then falls into three sections: "The political economy of nuclear power", "Nuclear power and employment" and "Political strategies". Pat Coyne recounts the chequered history of British civil nuclear development, identifying what he calls a trend to "state corporatism" and concluding that "nuclear power in the UK has been developed and kept alive by a combination of political will and institutional inertia. Economically it has always been seen in terms of future needs rather than present benefits, and the arguments for its necessity are open to serious doubts . . . the consensus of political interest which previously sustained it shows signs of cracking". Mike George delineates the details of ownership and control of nuclear industry; and Elliott asks whether—given the industry's sorry economic record to date—central planners find nuclear power appealing because it lends itself well to centralisation. Elliott then discusses past attitudes to nuclear matters on the part of trade unions in Britain, through the TUC Fuel and Power Committee, who "simply ask for more of everything—coal, oil, gas and nuclear", and sees this attitude gradually changing, as "wider economic and employment issues are gradually beginning to be raised".

The second section of the book dis-



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cusses health and safety and trade unions rights in the nuclear industry, and points to the need for more information. Consideration is then given to jobs, energy and industrial strategy, and alternative policy options of the kind recently detailed by Gerald Leach *et al.* in their report *A Low Energy Strategy for the UK* (Science Reviews Ltd and IED: London, £7.50; for review, see *Nature* 277, 162; 1979). According to Elliott, "Because the alternative technologies are mainly, by their nature, decentralised they can support a decentralised society, based on community and workers' control. But their introduction into society as it is clearly doesn't necessarily guarantee that such a state of affairs will come about". However, Elliott concludes, "The fight for alternative technologies is not an alternative to the fight for socialism. It can and should be part of it"—a sentiment making up in vigour what it lacks in grace. Part three of the book discusses how such aims can be pursued, by trade unionists and environmentalists working for common objectives.

All four authors write clear, readable prose, in language which is accessible

and blessedly low in jargon. However, it is a pity that more attention was not paid to fine detail; too many trifling but irritating errors of fact have survived into print. (There are eleven British Magnox stations in all, not twelve, including a total of 26 reactors; there is no power reactor in Denmark, only three small research reactors; Belgium, far from being a nuclear "poor relation", has three substantial power reactors already in operation and four more under construction, and generates a larger percentage of its electricity by nuclear power than any other country; the proposed commercial fast reactor would be 1,000 MW, not 100 MW; and so on.) It would be easy to correct virtually all these fluffs in a second printing. To do so would remove the most irksome blemish on what is otherwise a very useful book, one which deserves to be widely read and discussed, by trade unionists, environmentalists, and all those other 'ists who make up our disputatious society. □

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Peer review in the NSF

S. S. Blume

Peer Review in the National Science Foundation: Phase One of a Study. By the National Research Council. Pp. 193. (National Academy of Sciences: Washington, DC, 1978.) \$10.75.

Most readers of *Nature* will be well aware that the peer review system (the system by which research grant applications are evaluated by experts in the disciplines concerned) has recently been subjected to considerable criticism in the USA. Members of the general public as well as congressmen have attacked an 'old boy network' said to have control of research funds. The National Science Foundation has steadfastly refused to make confidential referees' reports (and their names) available to members of congress, and this has been held to be a concealment of the Foundation's biased procedures. Politicians have also accused the Foundation of giving inadequate weight to the needs of society (as distinct from those of science) in the selection of projects for funding, and

of discriminating against scientists in less prestigious institutions. The fault, some claim, lies in the peer review system. Ripples of these criticisms—the view that justice should not only be done but be seen to be done, that criteria of equity applicable in other areas of public resource-allocation should apply also in science—have reached these shores. For example, not long ago the UK Research Councils had to consider the claim that they dealt unfairly with scientists working in polytechnics.

This volume reports on the first stage of a study of the operation of the peer review system in the NSF, commissioned by the National Academy of Sciences. The researchers were given full access to reports and files on all applications to the Foundation. They interviewed programme directors (who unlike British committee secretaries have final responsibility for deciding which applications in their field get funded). In the study the authors focus particularly upon three things: How are reviewers (referees) chosen? How do the referees respond to various characteristics of the applicant other than their assessment of the quality of his (very rarely her!) application? And how does the final decision (that is, of the programme director) relate to the referees' assessments?

The study provides no ammunition to the critics of science. An effective combination of statistical and qualitative analysis is used to show, most

simply, that the best research gets funded. No evidence for an old boy network is uncovered, and findings suggest that referees are not much influenced by the status of the applicant's institutional affiliation, or by his seniority, or by whether he has or has not recently had a Foundation grant. They simply judge the application on its merits. Moreover, it seems to be these evaluations which do largely determine what gets funded. To be sure, if one tabulates 'successful' compared with 'unsuccessful' applications by extraneous variables such as those suggested above, one finds clear differences. But all of these vanish when reviewer's evaluation is introduced. Of course there are curiosities. A not infrequent one seems to be the eminent scientist whose application is rejected because it consists of little more than "I'm me—give me the money"! So the system seems to work as intended, and this study shows this to be so.

Nevertheless, as I am sure both authors and sponsors appreciate, the study will not silence the critics. It is not simply that the 'enemies' of science are unsusceptible to reason (though one wouldn't really expect them to succumb to a piece of applied social research). It is also because the study does not—perhaps could not—tackle all the criticisms made. For example, referees are required by the NSF to consider an application in the light of criteria reflecting the competence of the investigator, the scientific merit of the project, its relevance and utility, and its broad implications for American science and science education. The brief section of the report devoted to the problem of weighting these considerations against each other is all too sketchy, and will hardly persuade the cynic that much attention is paid to questions of practical applicability. One gets no feel for this at all. The critic can still claim that if the scientist from a poor southern college is 'fairly' treated, that is not enough. After all, positive discrimination in favour of the disadvantaged is an accepted feature of many areas of policy—why not in science? And even the favourably disposed but budget-conscious politician may feel uneasy in defending a government agency which makes "no systematic attempt to monitor the outcomes of research grants" (p. 158)—that is, to see how its money was spent.

But perhaps for the reviewer to attack a methodical piece of research for failing to resolve an essentially political dispute is not only naive, but inappropriately partisan. □

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Tribology textbook

Robert Schnurmann

History of Tribology. By D. Dowson. Pp. 677. (Longman: London and New York, 1978.) £35.

In 1964, the Rt Hon. the Lord Bowden of Chesterfield as Minister of State for Education and Science had asked Mr H. Peter Jost to set up a working party to report on lubrication research and teaching requirements. The academic world was represented on this working party by Professor Dowson. Before its findings were published in February, 1966, the "Jost Committee" had asked the editor of the Oxford Dictionary for a pithy name to summarise in one word the closely related subjects of Friction between Solid Bodies, Wear, and Lubrication, and he suggested 'Tribology'. Thus, HMSO published the report of the working party under the title *Lubrication (Tribology), Education and Research—A Report on the Present Position and Industry's Needs* which specifically mentioned "a conservative estimate" of the potential annual savings to British industry from improvements in education and research of "about £515 million, the biggest single item being savings in maintenance and replacement costs of £230 million".

This triggered off a spate of activity. Three centres of tribology were established in this country, two at Universities (Leeds and Swansea) and one in a government establishment (Risley). Tribology committees were formed, both national and international. Later, the Institution of Mechanical Engineers, which had already sponsored in 1937 a "General Discussion on Lubrication and Lubricants" and had followed this up twenty years later by another conference on "Lubrication and Wear" (organised jointly with the American Society of Mechanical Engineers), set up a Tribology Trust which has made annual awards (gold, silver and bronze medals) since 1972. Also in 1966, a news sheet called *Tribology News* was launched. A flood of publications and conferences was unleashed. 'Tribology' had arrived.

In 1969, Professor Dowson, who is the founder and director of the Institute of Tribology in the University of Leeds, decided to write a textbook on the subject with an opening chapter on its history. This 'chapter' grew to become the present book of 677 pages.

Looked at in a down to earth man-

ner, friction between solid bodies can either be unlubricated or lubricated, and in the latter case it depends on the operating conditions whether the film of lubricant is thick enough to sustain hydrodynamic lubrication—also called 'thick-film' lubrication—or whether one has to consider 'thin-film' lubrication—where the film of lubricant is not thick enough to allow of hydrodynamic flow. The word 'dry', in connection with unlubricated friction should only be used within inverted commas, because the surfaces of all solid bodies carry layers of one contaminant or another, either oxides or sulphides, or adsorbed or condensed matter. Even if these were removed by volatilisation in an ultra-high vacuum, recontamination of the surfaces, which were made 'naked' by volatilisation, would commence instantaneously.

The case is not analogous to the experiments of Agnes Pockels and Irving Langmuir, who scraped and thereby created a 'naked' water surface simultaneously with the application of an organic liquid. This is not the way in which Professor Dowson has set the scene. He would probably agree, however, that friction between solid bodies is a very complex phenomenon. The 'Jost Committee' rightly said that it is "multi-disciplinary". This has been very little heeded in the literature to which Professor Dowson refers. Even when he approaches the end of his journey through the ages and turns to his own subject of Bio-tribology, he uses the misleading word "cold-welding" for what is in fact the adhesive action of films so extremely thin that internal shear in these films is no longer possible (Lord Rayleigh's celebrated 'cup and saucer' experiment).

Throughout the book there is a great deal of confusion between adhesion and cohesion. The wide use of Johansson thickness gauges does certainly not support Professor Dowson's claim of the "cold-welding" of like metals, with-in the true meaning of the term 'welding' as an interpenetration of crystal grains. True, each gauge block is coated with lanolin before they are wrung together, but the process of wringing them together is necessary to make the residual film thin enough for strong adhesion. In this context, Amontons (p154) had coated his blocks of either copper, or iron, or lead, or wood with cart grease, not necessarily with "old pork fat". Amontons himself said "... enduits de vieux-oingt". The other blocks "of similar materials and different sizes" which were loaded against the greased ones were ungreased. Nowhere is there any mention of the significance of Amontons' Law as a wear criterion and its importance for the safe operation of machinery.

Neither is it Professor Dowson's thesis that it now seems that Sir William Hardy's introduction of the term 'boundary lubrication' was perhaps unfortunate, particularly if it is used to differentiate qualitatively between materials which function by the same mechanism though with marked quantitative differences, such as additives for so-called 'boundary lubricants' and additives for 'extreme pressure lubricants'. Similarly, referring to the chapter on wear, the important qualitative difference is between "mild wear" and "catastrophic wear" and not between "adhesive" and "abrasive wear".

Professor Dowson has included in his account 16 pages of history of the petroleum industry and of the origins of the major oil companies. John D. Rockefeller was indeed a dominant figure for the greater part of his life. He became a philanthropist at the end of his business career, and for many years, the Rockefeller Foundation did a great deal for the promotion of some outstanding scientific research by enabling promising young scientists to spend one year's leave of absence working with people, such as Lord Rutherford, Niels Bohr, Enrico Fermi, Otto Stern, to name but a few.

The petroleum industry discontinued some years ago the use of 'a-hole-in-a-pot' viscometers, such as the Redwood, or Saybolt, or Engler, and nowadays uses glass capillary viscometers. Incidentally, the c-g-s unit of kinematic viscosity is "one Stokes" and not "one stoke". The SAE classification of lubricating oils concerns their viscosity levels and has nothing to do with the dependence of their viscosity on temperature. Even Dean and Davis's 'viscosity index' (VI) only *compares* the viscosity-temperature dependence of a lubricating oil with that of two other lubricating oils, one of which is a Pennsylvanian oil which has a comparatively very flat viscosity-temperature curve, whereas the other one is a Gulf Coast oil which has a relatively very steep viscosity-temperature curve. Therefore, the VI-numbers assigned to these two types of comparison oils were arbitrarily taken as respectively 100 and 0. This was an important marketing asset in the days when only one of the American oil companies had a monopoly on Pennsylvanian Brightstock, which was added to petroleum distillation cuts and raffinates as a VI-improver. With the advent of mineral oil-soluble polymers, values of VI greater than 100 could be attained, and the Dean and Davis scale cannot be used here. For these values of VI above 100, W. A. Wright constructed a different VI-scale. The *Standard Methods of Testing Petroleum and its Products* of the Institute of Petroleum, the American Society for Testing Ma-

terials, and ISO have therefore given since 1966 "two procedures" for calculating VI, one for values between 0 and 100 and, if calculation should indicate a higher value, another one for values above 100.

Professor Dowson explains in his introductory chapter the chronological "Structure of the book", which is divided into 11 chapters, and his belief in "the wisdom of the 50-year rule", in connection with chapter 11 which has the heading: "Towards tribology: 1925—the present". The "50-year rule" is Professor Dowson's own variation on someone else's comment that "... a wise historian usually stops 20-30 years before his own time, because he cannot see the wood for the trees; the so-called thirty-year rule". The book has an appendix of 25 biographical sketches which were modelled on a book by one F. R. Archibald.

The book is remarkably free from misprints. There are the odd ones, but they are few and far between, although it is a little startling to find on p443, for instance, -11.8°C as the conversion of 0°F , when the right figure of -17.8°C is given 5 lines further down; that 121.1°F is given on p30 instead of 119.1°F as the conversion of 49.5°C is less easily explained; nor that 0.001 oz is said to be equal to 0.038 g, whichever kind of ounce it may be. But these are details. What is disturbing are the many mistakes both in the body of the text and in the "chronological tables".

Bourgeois chemist

Colin A. Russell

Gay-Lussac: Scientist and Bourgeois. By Maurice P. Crosland. Pp. 333. (Cambridge University Press: Cambridge, 1978.) £15.

DECEMBER, 1978, was a busy one for chemical historians. The bicentenary celebrations of Humphry Davy's birth were followed by similar festivities in honour of the French chemist, Joseph Louis Gay-Lussac. Both men were born in December, 1778, though in the country of his birth Davy is much better known. Although equally distinguished for his chemical work, Gay-Lussac has had to wait until his bicentenary for a full length biographical treatment in English. The urgent need for such a book has now been met by the timely appearance of this study by Professor Maurice Crosland.

To most modern chemists, Gay-Lussac is merely a name associated

This is not the time and place to list them all. A few randomly chosen examples must suffice here. Lord Rutherford discovered the atomic nucleus in 1911. In 1919 his great achievement was the 'splitting of the atom'. He coined the name 'proton' for the positive hydrogen ion because, before the discovery of the neutron in 1932, he considered protons and electrons to be the elementary particles of the atomic nucleus. C. T. R. Wilson's cloud chamber dates from 1897; and 1926, not 1924, is the date for Schrödinger's wave mechanics. The name of the discoverer of X-rays was Wilhelm Conrad Röntgen and not "Wilhelm von Röntgen". Sir William Hardy and Philip Bowden were members of the Senior Common Room of the same Cambridge College, Gonville and Caius.

The chronological tables are not without their own mysteries. In the juxtaposition of "Political and social events", "General, technical and scientific developments", and "Tribology", one wonders why the "Tribology" column was chosen to record such events as Henry Deterding assuming "control of Royal Dutch Company", the "merger between Royal Dutch and Shell", and the move of the Timken Company to Canton, Ohio. □

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with two or at the most three notable achievements, the best known of which is his Law of Combining Volumes (1808), asserting a simple ratio between volumes of combining gases and their products (if gaseous). He has also been associated with the law connecting gaseous volume with temperature, though more frequently this is referred to as "Charles' Law"; in fact Gay-Lussac was the first to publish (1802), but his experiments were based on earlier work by J. A. C. Charles. Thirdly, chemists will recall the "Gay-Lussac tower" as part of the lead chamber process for making sulphuric acid, being a device for recovering nitrogen oxides from the effluent gas. The last Gay-Lussac tower that I know of in this country disappeared when the final lead chamber plant was dismantled about four years ago, so a further association with Gay-Lussac will be lost to future chemists.

However, as Crosland has been at pains to show, Gay-Lussac is worthy of commemoration for much more than this. His Law of Gaseous Volumes not only provided the foundation on which Avogadro and Cannizzaro were later to build their own theoretical structures; it seems that it confirmed to its

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discoverer the fundamental importance of volume measurements—even greater than that of weights. This conviction was nurtured by Gay-Lussac's respect for his mentors Berthollet and Laplace, whose concern for establishing general laws in science proved more attractive to their young disciple than the mere multiplication of new compounds, rather in the old natural history tradition. Oddly enough, recognition of the Law seems to have been unexpected and came (as did many other generalisations in nineteenth-century chemistry) from studies of relatively obscure and complex phenomena, in this case the reaction between ammonia and boron trifluoride.

Associated with Gay-Lussac's work on gaseous volumes was his recognition of vapour densities as accessible and significant parameters for a compound. They were prominent in his researches on the new substance iodine. Gay-Lussac studied it extensively, concluding it to be an element similar to chlorine. He described and analysed hydrogen iodide, and discovered iodic acid, two chlorides of iodine, iodoethane, and so on. Closely related to this work was his research on cyanides. He obtained "prussic acid" in a pure state and prepared cyanogen for the first time. Wavering in his patriotic devotion to the memory of Lavoisier he recognised that oxygen need not always be present in acids, and to cover hydrogen halides and cyanide introduced the term "hydracid".

Nor was Gay-Lussac's concern with volumes confined to gases. In a largely industrial context he pioneered analytical methods for solutions. To have "introduced scientific rigour and established volumetric analysis as a branch of chemistry" was no mean achievement. By fitting a vertical side-tube to a graduated cylinder he devised a primitive burette that may still be occasionally encountered (though it soon turned out to be too fragile for the industrial purposes for which it was designed). He developed the pipette to something like its present form, introduced arsenic (III) as a standard for chlorimetry and made many further innovations. Other scientific work ranged from a study of fermentation to the development of lightning conductors. He studied meteorology and heat, and became one of the few Frenchmen to follow up the discovery of the Voltaic pile.

Gay-Lussac's chemical research is described in this book with verve and skill. The historian will delight in the clarity with which arguments are marshalled and the degree of historical analysis that enables his work to be seen within the context of contemporary science. Chemists will welcome a historical account that actually takes

the trouble to describe the chemistry being done—an increasing rarity these days. They may cavil at a nomenclature that sometimes is neither ancient nor modern and at the use of "react" as a transitive verb, but these are small blemishes in an account that takes seriously the scientific content of Gay-Lussac's lifework.



Gay-Lussac

For all his obvious sympathy and respect for Gay-Lussac, the author does not portray him as either saint or hero. Rather charmingly he includes in the index an entry "Gay-Lussac, Joseph Louis, mistakes of"! It has five references. Rightly he does not classify the rejection of atomism as a "mistake", though with hindsight it might be said to have been a pity. In Gay-Lussac's thinking there was a streak of pre-Comtean positivism later to find fuller expression in the uncompromising anti-atomism of Berthelot. This reluctance to go beyond experimental data taken in conjunction with Gay-Lussac's predilection for general laws suggests a tension which is never completely resolved.

Gay-Lussac is presented as a product of the middle classes determined at very least to maintain his social position. This resolve seems to have been a mainspring for his lifework, leading him to accept a plurality of academic posts, to develop his consultancy practice and to do all he could to improve his financial position. Two bar-charts illustrate that as his productivity (in terms of scientific papers) declined, so his salaried income rose to a peak. It is interesting to contemplate this recipient of a 50,000 Fr. salary insisting on patent rights for his

"Gay-Lussac tower" (and receiving 150,000 Fr. as his reward), while a self-taught plumber on Tyneside declined to patent the very device that made the Frenchman's invention a practical proposition—the "Glover tower". However some may wonder whether financial gain was all that being a "bourgeois" meant for Gay-Lussac's science. Did "bourgeois" attitudes colour the solution as well as the selection of scientific problems? Was his commitment to laws of nature (for instance) a reflection of middle-class convictions about the world? On these matters the author says little, probably for the good reason that there is little that can be said with any confidence, although he does allude to Gay-Lussac's ambivalent attitude to metrication, when "bourgeois" and "scientific" values were in conflict. He also gives a brief account of the chemist's activities as a Member of the Chamber of Deputies and his typically protectionist attitudes to industry and (especially) agriculture.

Gay-Lussac's institutional affiliations give his biography special interest for historians. He held posts in the Ecole Polytechnique, the Paris Faculty of Science, the Muséum d'Histoire Naturelle and the Ecole d'Application des Tabacs; he was a member of the National Institute and of innumerable official committees; he was joint editor of *Annales de chimie et de physique*; he became assay master of the Mint and was closely associated with the chemical plant of the great Saint-Gobain Company; above all, his early years were spent in the unofficial scientific society that met at Berthollet's home at Arceuil. Such a wealth of association gives the author an opportunity to place his subject in an institutional context of extraordinary breadth, and this he does remarkably well. Quite apart from the importance of Gay-Lussac himself, this biography gives a valuable insight into French scientific institutions of the early nineteenth century. Because of Gay-Lussac's specialist training in science, and because he subsequently earned his living through its practice, Crosland regards him as "one of the first generation of professional scientists". One wonders how useful a category of analysis this may be for an individual in early nineteenth century France, in view of the ambiguities lurking within the concept of 'professionalisation'. It may suggest false analogies between different countries or different periods. Only when such a term is rigorously defined does it become meaningful, and precisely at that moment it becomes redundant. If professionalisation is seen at the time to be a crucial issue then that of course is a different

matter, but this does not seem to have been the case with Gay-Lussac.

The author declares "this biography will help give flesh and bones to one of the names found in science textbooks but, in so doing, it may also make a small contribution to a fuller understanding of the development of science". I believe that he is both over-sanguine and too modest. For me, at least, Gay-Lussac never emerges as a rounded human personality. Here and there we catch glimpses of a somewhat unbending autocrat tending to inspire respect rather than affection and himself under pressure from a financially ambitious wife. But in fact we learn little of his family life, his hobbies ("no great interest in the arts") or his religion (Enlightenment humanism). We do not even know the circumstances of his death. On the

other hand, for science itself the book makes more than the "small contribution" suggested by the author. It is a solid piece of research meticulously executed with tables, footnotes, bibliographies, appendix and indexes, and is a substantial addition to our understanding of the development of chemistry at an important stage of its history. At £15 one might have expected the publishers to allow the author more illustrations than the solitary portrait that appears as the frontispiece. But we must be grateful to Gay-Lussac for having contributed so much to chemical science and to Maurice Crosland for an illuminating and soundly-constructed essay in scientific biography. □

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Salt in perspective

Frank Greenaway

Neptune's Gift: A History of Common Salt. By R. P. Multhau. Pp. 325. (Johns Hopkins University Press: Baltimore and London, 1978.) £15.75.

"SALT was known to the Ancients". So began for fifty years the lectures on the halogens given by an imperishable Oxford don. Robert Multhau might well have begun his excellent book with the same words, conveying as they do the antiquity and continuity of his subject. A book which is primarily historical in intent, but concludes with a reference dated 1976 is, to say the least, unusual, but Multhau is an unusual man.

He has been a man of action and saw the world before entering on a career at the Museum of History and Technology at the Smithsonian Institution which made him so well known as a leading historian of chemistry. His reach is long and his grasp firm. This work on salt has all the fluency and density of significant detail which make his earlier work *The Origins of Chemistry* so valuable.

The book falls into two main sections: "The Age of Culinary Salt" and "The Era of Chemical Salt". The transition is set later than would seem obvious—that is, in the nineteenth rather than the seventeenth century. Multhau sees the food preservative role of salt as its more important feature well into the nineteenth century. Only with the expansion of the

chemical industry as a whole and the concomitant expansion of large-scale manufacturing industry and long distance bulk transport does he depict salt as a key chemical raw material.

Salt is a universal necessity. Without an adequate intake, direct or indirect, normal health cannot be maintained. Yet salt is not so widespread a natural substance as might seem from its invariable association with animal life. It has to be sought by human and animal alike; so since ancient times, it has had the scarcity value which makes for trading and taxation. A history of administration could be written around salt, "an ideal article for fiscal management". There was a universal demand for it, its supply could not be left to chance and it was a fairly easy commodity to control, as was found not only in Europe but also in China and elsewhere. The salt tax, the *gabelle*, was hated in France and seems to have been one of the precipitating causes of the French Revolution. However, one must not take too simple a view of the historic role of salt taxes, which came and went in a very irregular way in the United States, and were much confused in Indian history by the close relationship of salt with the important saltpetre industry and trade.

The extraction of salt is an exceptional example of the stability and continuous survival of techniques which, though crude and simple, were efficient enough to render modification unnecessary for centuries. There were several methods, some of which might be found in use within a short distance of each other: evaporation of seawater by fire or solar heat, surface mining, deep mining, leaching, and so on. Simple pottery pans for boiling down brine were so customary that their fragmentary remains are good evidence of the

existence of a salt industry. The largest salt industries, however, were eventually those based on deep-mined rock-salt, the demand being that of the new chemical industries. (One of several sections contributing to the wider history of technology is that dealing with drilling.)

Before salt entered its modern phase of being the raw material for sodium compounds and for chlorine, it was one of the most important means of sheer survival, being an invaluable food preservative. Lynn White has drawn attention to the importance of improved second-class protein supplies to the development of mediaeval technical vigour, but the argument can be continued: new technical improvements in salt manufacture improved the availability of meat and fish protein and so progressively helped to bring about the energising of Western Europe which ushered in what we can still usefully call Modern Times. But even greater expansion was to come as salt-mining benefited from the steam-engine and the development of iron machinery. Multhau uses an enlightening analogy: comparing early salt-mining to an agricultural pursuit because of its often seasonal character, and showing how it was transformed into a non-seasonal industry. By 1850 the alkali industry took up about a third of British salt production. In the United States, the new meat-packing industry called for a great deal, but industrialised food preservation was much less important elsewhere. Direct culinary use levelled off about 1850, using about half of the British total at that time. The industry expanded, however, and the second half of the book explains why. The nineteenth-century chemical industry had soda as one of its pillars with salt as the base, and the vast increase in the use of salt-derived chlorine in solvents, plastics, pesticides and the like. Multhau points out how in the case of salt, as with so many other substances, the dominant industry is the automobile industry, using the plastics and solvents, which need chlorine, in direct manufacture, but also using very large amounts of salt as such for road treatment in winter.

The book is well produced, particularly well documented, and equipped with valuable maps and statistical tables.

Salt was known to the ancients, but, in spite of its importance, is little considered today in the perennial discussion of the effects of technology. *Neptune's Gift* should do much to equip us to see it in its correct perspective. □

Frank Greenaway is Keeper of the Department of Chemistry at the Science Museum, London, UK.

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Shifting paradigms in chemistry

Maurice Crosland

Chemistry Transformed: The Paradigmatic Shift from Phlogiston to Oxygen. By H. Gilman McCann. Pp. 179. (Ablex: Norwood, New Jersey, 1978.) \$14.95.

THIS book is in many ways a pioneering study. Using T. S. Kuhn's idea of 'scientific revolutions' Dr McCann has spent more than ten years analysing one particular case. It is comparatively easy for the non-specialist to understand the main features of the so-called 'Chemical Revolution' of the eighteenth century, and a succession of philosophers of science have used it to illustrate their views about theories, falsification, and so on. This is the first time that a sociologist of science has tackled this historical question and we must respect the author's study of the relevant chemical journals and a wide range of secondary literature. This is no superficial survey but an attempt to achieve a better understanding of what really happened. The author also touches on the beginnings of the professionalisation of chemistry in France. With his great industry in statistical analysis of his data McCann deserves to succeed in saying something new and important. Certainly his approach opens up several interesting perspectives.

It is difficult to say exactly when Lavoisier's oxygen theory came to take the place of phlogiston. McCann marshals his data into periods: 1772-77 being the period during which Lavoisier was developing his theory and 1778-84 the years when Lavoisier was trying to justify his theory to others and meet their objections. 1785 was the year of Lavoisier's experiment on the composition of water which helped win over Berthollet, with other major chemists soon to follow. In Britain conversion to the new views was slower, and much ingenuity is exercised by the author to explain this. Rejecting nationalism as a possible ground, McCann believes that the British "chemical community" (if there was one) was more elderly than the French and therefore more conservative. Indeed the importance of age is claimed to be one of the great 'discoveries' of the quantitative sociological analysis used. But the author passes far too

easily from a plausible hypothesis about receptivity to one about innovation. It is claimed that what made Lavoisier unique was not (as has been assumed) his genius but his comparative youth, being only 29 at the time of his vital work which recognised the gain in weight of phosphorus on combustion. But what about other French chemists who were about the same age? One might mention the provincial chemists Cornette (28) or Opoix (27). Of course these did not have the advantage of living in Paris, of being members of the Academy, or of having a special interest in gases. But if age was so important in being open to change why is Lavoisier's exact contemporary Opoix remembered (if at all) as a late champion of the phlogiston theory?

Perhaps after all one needs to examine various other factors including

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Antoine Lavoisier

the arguments used on both sides, an aspect glossed over in the sociological treatment. The superiority of the oxygen theory is assumed and it is never mentioned that this 'correct' theory whose acceptance we are analysing included the (incorrect) 'caloric'. This is particularly serious in so far as it affects the very concept of a 'paradigm shift' mentioned in the subtitle, as it has been suggested that in some ways 'caloric' was phlogiston under another name. On p96 there is a remark about "the period of crisis as the French chemists solved problems that stymied the phlogistonists of Britain" but there is no hint that the phlogistonists provided problems such as that of 'heavy inflammable air' which were insoluble in terms of the oxygen theory until Cruickshank in 1801 showed that this was carbon monoxide.

The attempt to gain new knowledge

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by a quantitative approach is worth serious examination and it is a pity that this brave beginning is open to a wide range of objections, of which one is the small size of the sample used. Thus in the period 1778–80 one new author could write 20% of the articles cited. The problem of the disproportionate number of articles written by Lavoisier himself is fully appreciated by the author, who presents data in alternative forms to meet this objection. There is also the problem of biographical detail for some of the lesser well known chemists which might affect the picture of the 'average chemist'. Finally it is

not clear to the reviewer how we can count "oxygen articles" in journals before the discovery of oxygen.

I am sure that a careful use of a quantitative approach may provide valuable new insights in understanding the development of science but it will be all the more useful if it is brought in as an additional tool rather than as an alternative. In counting heads let us not ignore what was in those heads.

Maurice Crosland is Director of the Unit for the History, Philosophy and Social Studies of Science at the University of Kent at Canterbury, UK.

Respecting aberrant figures

J. R. Ravetz

Man and Nature in the Renaissance. By A. G. Debus. Pp. 159. (Cambridge University Press: Cambridge and London, 1978.) Hardback £7.95; paperback £2.50.

THE history of the science of the period of the Renaissance is directly relevant to the practice of science today, for our present ideas of what constitutes genuine science, and what demarcates it from false imitations, were forged then. For a very long time it had been common for historians to take the evaluations of the seventeenth-century innovators as simple fact: those who adhered to older methodologies of world-views were simply reactionaries, fools or charlatans. Scientists who achieved great science (by our standards) in spite of such defects (as Kepler or Gilbert) were explained as being "strange mixtures of the old and the new".

In recent decades, there has been a thin but growing stream of research

that treats some of the aberrant figures with respect, both for their stature in their own times and for such 'positive' achievements as can be gleaned from their work. The most promising candidates for such re-evaluation are in the tradition of alchemically tinged practical chemistry, relating directly to metallurgy, medicine, and having implications for philosophy, religion and politics. The leading figures in that 'chemical philosophy' were Paracelsus and van Helmont; its leading interpreters Page, Rattansi and Debus.

Now this school has produced an introductory textbook by which students are shown how much more there was to the scientific revolution than the developments centring on the work of Galileo. In it we find brief and illuminating accounts of the 'chemical philosophy'. Otherwise, it is a competent survey of developments in science and in methods in the sixteenth and early seventeenth centuries. As it is read and used, we may expect that the 'chemical philosophy' will achieve a sort of respectability among those who learn or teach the history of science.

This is a very valuable achievement. A most urgent educational task is to show that the Galilean style of reductionist, abstracted research is not the uniquely true approach to a proper knowledge and control of nature. A modest and uncontroversial introduction to a broader conception of science

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will have a greater diffusion in teaching establishments than a strident polemical tract. But a price is paid for this very Fabian approach to intellectual reform; there is a loss, not so much of ideological militancy as of historical drama. Our sort of science was conceived and propagated by men with a prophetic mission. The issues in their debates ranged over all human values and experience. Different conceptions of reality were in open collision then, and ideological commitments (based on a fusion of political and theological concerns) could be as important as experimental facts in determining positions in the still immature natural sciences of the time. Of all this, we get only the most mild of hints. The received historical doctrine of the rise of modern science as an automatic consequence of the adoption of correct attitudes and methods, is here not so much challenged as modified at the fringe. But perhaps that is appropriate for a pioneering venture in textbook writing; and as a simple, lucid and accessible introduction to its subject, the book succeeds well. □

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Wheelwrights of the heavens

A. W. Sleeswyk

Geared to the Stars: The Evolution of Planetariums, Orreries and Astronomical Clocks. By H. C. King, in collaboration with J. R. Milburn. Pp. 442. (Adam Hilger: Bristol, 1978.) £25.

In twenty-one chapters and over 366 pages, illustrated with 333 diagrams, reproductions of engravings and photographs, sprinkled with 2,236 footnotes, this book gives an account of planetariums, tellurians, orreries, globe clocks, astrolabes, lunariums, astronomical clocks, equatoriums, satellitians, cometariums; all of them, to the modern eye mechanical analogue computers of the Solar System, but differing in the way the data are displayed. These models—often of great beauty—are now, as are all analogue computers, being superseded. At the end of the book the most modern planetarium is described, with the heavenly bodies represented by spots of light projected on the interior of a dome vaulting over the audience, and controlled by a digital mini-computer.

Astronomers, who often earned their keep as astrologers, have observed from the earliest times onward the recurrent motions of the heavens, and the bodies moving in them—Sun, Moon and planets. Whether or not awed by “the infinity of those spaces”, they determined with ever-increasing accuracy the different periods of these motions, whose ratios are never exactly expressible as finite quotients. Hence the challenge to clever craftsmen to produce close approximations by ingenious gearing.

The material is arranged in a roughly chronological order, starting with the little we know about Hipparchos and Archimedes in connection with astronomical models—the protohistory of the subject. Under-water archaeology brought to light the next item, the astonishing Antikythera mechanism, probably made by a Rhodian craftsman in 87 BC, lost in a shipwreck north-west of Crete in about 80 BC, and salvaged by sponge-fishers in the year 1900. Its 30-odd gears were conserved in lumps of calcareous accretion, which only in recent years became amenable to analysis, by X-rays and gamma-rays, on which D. J. Price in 1974 could base the definitive interpretation. The mechanism computed the synodic month and the lunar year and

the positions of Sun and Moon on the basis of Meton's (about 430 BC) approximation, in which 19 solar years correspond with 254 sidereal revolutions of the moon, or 235 lunations. As many later geared models, it made use of differential gearing.

Fourteen centuries passed before anything nearly as elaborate was made. The interim is filled by the Arabs with their geared astrolabes, and the Chinese with the waterwheel clock made by Chang Ssu-hsün exactly a millennium ago. Later, this clock was perfected by Su Sung, and lost as a link in the development of technology when the Chinese capital K'aifeng fell to the Chin Tartars in 1126.

The first mechanical clocks date from the end of the thirteenth century, and the equal hour system gained universal acceptance only a century later. Not surprisingly, the first clocks about which we possess detailed information, those of the fourteenth century of Richard of Wallingford and Giovanni de' Dondi, were astronomical clocks. The positions of the Sun, the Moon and the five planets were displayed on separate dials in the latter clock, which is adequately discussed. Regrettably, the same cannot be said for the discussion of Richard of Wallingford's clock, reconstituted by J. D. North and published in final form in 1976: it is unnecessarily brief and marred by errors of detail. Astronomical clocks have been with us ever since those early examples were made.

Astronomical modelmaking received some powerful impulses from developments in cosmography. In 1543 Copernicus published his *De Revolutionibus*, replacing Ptolemy's geocentric Universe by the much more orderly heliocentric one. In 1609 appeared Kepler's *Astronomia Nova* on the properties of the elliptical planetary orbits, providing a new basis for the “equation of time”, that is, the difference between sundial time and mean solar time, indicated in many astronomical clocks. In the course of time the production of these display clocks diverged increasingly from scientific clockmaking, which aimed at greater accuracy, culminating in Harrison's marine chronometer of 1735.

The trail of astronomical clock making brings us back to the late sixteenth century, to the court of the landgrave William IV of Hessen-Cassel, who employed some masters of the craft, Eberhard Baldewein and Jost Bürgi; then to England in the seventeenth and eighteenth centuries, when Tompion, Mudge and Pinchbeck made clocks catering to the tastes of educated gentlemen of the time. Astronomical clocks of heroic complexity were made in Central Europe: for instance, by

Aurelius a San Daniele and David a San Cajetano at the royal court monastery at Vienna during the second half of the eighteenth century; above all, perhaps, the ‘World Machine’ finished by Ph. M. Hahn in 1791. Schwilgué made the astronomical clock in Strasbourg in 1842, more or less in this tradition. His workshop was the germ of the firm of Ungerer, makers of a most elaborate clock for the cathedral of Messina in 1933.

The line to be drawn between astronomical clocks and other astronomical displays is, of course, vanishingly thin. In France a development occurred of clock-driven armillary spheres, of which Antide Janvier finished an outstanding example in 1801. In England, orreries were mainly developed as aids in adult education by instructors such as James Ferguson (1710–76), who, as “Wheelwright of the Heavens” has a chapter devoted to his life and work. This development on the home-front of astronomy gave rise to transparent demonstrational orreries for large audiences, and, therefore, of maximum size—a development which ultimately led to the projection planetarium.

This work germinated a quarter of a century ago as a modest project to compile a list of extant models of the Solar System. In 1970 its scope expanded so as to include astronomical clocks and biographical notes. The result bears traces of this gestation. It is a somewhat uneven, but readable account of the elements of the early history of astronomy, horology and instrument making for the interested amateur, to whom the geometry of the various models is explained especially well. On the other hand, the historian of science will find this book useful too, because it has retained its original character of a catalogue; none other exists. He will recognise some obvious shortcomings—rather heavy reliance on secondary literature, occasional inaccuracies and the absence of a firm terminus to the references. As stated before, the findings of J. D. North—also referred to as J. and J. G. North—on the clock designed and executed by Richard of Wallingford—also referred to as Robert—are rather inaccurately reproduced. The book by Leopold of 1974 on the Reinhold astronomical table clock is not among the references, nor is Maurice's authoritative book on German geared clocks, although an article by the same author in the September issue of the *Connoisseur* of the same year (1976) is included.

These, however, are only minor flaws in this wide-ranging book, which offers much at a reasonable price. □

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Meticulous archaeological surveys

J. E. Wood

Megalithic Remains in Britain and Brittany. By A. Thom and A. S. Thom. Pp. 192. (Clarendon/Oxford University Press: Oxford, 1978.) £8.95.

THE senior author of this book, Alexander Thom, is a mechanical engineer and was Professor of Engineering Science at Oxford University for 16 years until his retirement in 1961. For 35 years or more he has devoted much of his life to the study of the many stone settings which are found in the wilder parts of Britain and in Brittany and date from the late Neolithic and Early Bronze Ages. Thom is not an archaeologist and his research is done, not with the spade, but with the steel tape and the theodolite. He has applied his scientific training to the precise measurement and surveying of these remains. His work was originally published in scientific papers, but at intervals he has collected his results together for publication in book form. *Megalithic Remains in Britain and Brittany* is the third book; the earlier ones, *Megalithic Sites in Britain* and *Megalithic Lunar Observatories*, were published in 1967 and 1971.

The new book was written jointly by Alexander Thom and his son Archibald S. Thom, who has been actively working with his father for many years. About 80% of it is based on a series of papers which were published in *Journal for the History of Astronomy* between 1971 and 1977. Some of the paragraphs and most of the diagrams are indeed identical to those previously published in the *Journal*, but apart from the convenience of having Thom's results in one volume, their work gains in significance when it can be studied as a whole. The book is particularly valuable for giving a comprehensive view of their work on the Carnac megaliths; this is the main topic and accounts for about one third of the text. They describe their surveys of the stone rows, give geometrical interpretations and postulate the existence of two prehistoric lunar observatories. Avebury, Stonehenge and the Ring of Brodgar are also analysed in detail, and there are short notes on several lesser sites. There is also a chapter on the geometry of cup and ring marks.

One's first reaction on reading the book is a feeling of admiration and

awe. The whole of its contents is the original work of one dedicated man and his team of assistants. Only someone who has walked along the stone rows of Carnac and Kermario, and seen the vast numbers of stones, the almost impenetrable thickets of gorse which cover the smaller ones and the clouds of voracious midges in midsummer, can appreciate the enormous amount of hard work and discomfort that underlies the Thom's detailed plans of the alignments. They have raised the standards of archaeological survey work to a far higher level of accuracy than earlier surveyors of stone rows, and the archaeological community should be ever grateful.

In the course of his studies, Professor Thom arrived at three conclusions: (a) that the non-circular stone rings were not merely accidental departures from circles but were intentionally laid out in other geometrical figures, such as ellipses, flattened circles and egg-shapes; (b) that a precise unit of length, the Megalithic yard of 2.722 feet, was used in setting out the stone rows and circles, over the whole area from Shetland to Carnac and for the whole period of Megalithic buildings; and (c) that the function of many of the stone settings was astronomical, either indicating sightlines for observations of the azimuths of the rising and setting points of the Sun and Moon, or as calculators for predicting lunar eclipses.

Thom's conclusions have been hotly debated by archaeologists for more than a decade, and are not fully accepted at the present time. (Many archaeologists, for example, would accept the idea of a local unit of length, but not the universality of Thom's proposal.) In their book there is barely a hint of the controversy. The starting point is the assumed correctness of the elder Thom's ideas, and sites are individually interpreted in that light. There are no alternative explanations and in the minds of the authors there are no doubts.

Thus, the authors' approach is largely to disregard conventional archaeology, and they do not discuss whether their conclusions are reasonable in the light of current archaeological knowledge. The stone circle at Avebury, for example, is analysed by the Thom's as a compound ring consisting of six circular arcs drawn from six centres which are geometrically related to a 3, 4, 5 Pythagorean triangle. It is the most complicated ring in Great Britain. However, archaeologically there are grounds for believing Avebury is earlier than most rings in the British Isles, that early rings were geometrically simple, and that the more complicated rings are the later ones. The Thom's interpretation does not fit

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comfortably into the archaeological framework, and archaeologists are bound to ask if simpler constructions are possible. The robustness of their interpretation, such as the possible range of values for the radii of the arcs, is not discussed.

Another example of their conclusions being in conflict with the archaeology is the supposed distant foresight at Stonehenge, called 'Peter's Mound', which was suggested as an indicator for midsummer sunrise. This looked unlikely archaeologically for several reasons, and indeed was shown by excavation in 1977 to be modern, though this fact does not appear in the book.

These criticisms are not meant to detract from the authors' work, for which I personally have considerable

admiration, but are intended to indicate the nature of the book. It is not a balanced account of megalithic astronomy and geometry, but a compilation of original work, the field reports of a team of meticulous surveyors, to which the serious student of the period will turn and which no specialist in the late Neolithic and Early Bronze Ages can ignore. Some of their interpretations may be proved incorrect in the course of time, but this is not particularly important; *Megalithic Remains in Britain and Brittany*, together with Alexander Thom's two earlier works, will be an invaluable source book for researchers for very many years. □

J. E. Wood is a Deputy-Director of the Admiralty Surface Weapons Establishment, Portsmouth, UK.

Star formation

Bart J. Bok

Stellar Formation. By V. C. Reddish. Pp. 287. (Pergamon: Oxford, 1978.) Hardback £15; paperback £7.50.

THIS is an interesting volume and I learned much from reading it with care. At the same time, I express deep regret that it came out in print effectively four years after the writing was finished. It covers adequately and thoughtfully the literature available to the end of 1974 and should therefore have been published by the end of 1975. It does not include the new results reported at IAU Symposium No. 75, which was held at Geneva in September, 1976 (International Astronomical Union Symposium No. 75, *Star Formation*, D. Reidel: Dordrecht, Netherlands, 1977), nor does it include researches that were discussed and analysed at the Tucson Colloquium of January, 1978, papers that are now in print (T. Gehrels, *Protostars and Planets*, University of Arizona Press: Tucson, 1979; *The Moon And The Planets*, 19, No. 2, 1978).

There are several very good chapters in the present volume. Chapter 3 deals in a masterly fashion with the determination of the initial luminosity function and the initial mass function. The three chapters that follow it are a bit on the meagre side, but the concluding chapter of the observational

half of the book gives a well-rounded review of the necessary environment for fostering star building from interstellar clouds. Professor Reddish defends strongly the point of view that star formation results almost exclusively as a by-product of the formation of H₂ molecules on very cold solid particles in interstellar clouds. I agree that this is a major contributing process, but there are regions in the heavens, notably in the Magellanic Clouds, in which cosmic dust particles and even H₂ molecules seem to have played less of a part than neutral atomic hydrogen.

Professor Reddish's independent and honest scholarly considerations have led him to visualise ahead of his time one major process contributing to star formation, but he ignores others. If I were asked to list my favourite processes for star formation, I would list five, which seem to be at work in our galaxy within 1,500 parsecs of the Sun. These are: (1) The formation of OB stars within large dark complexes such as the Ophiuchus Complex, the Taurus Complex, the Greater Orion Complex and the southern Gum Nebula. (2) The Elmegreen-Lada process according to which a large molecular cloud acts as a feedbag for the continuous formation of OB stars in the vicinity of established H II regions and associated young OB groupings such as Messier 8 and Messier 17. (3) Supernovae and pulsars, whose high-energy charged particle emissions, along with the Lin-Shu spiral density waves, contribute materially to the condensing of interstellar clouds, thus leading to star formation. (4) Cloud-cloud collisions and interactions between passing clouds, which may further assist condensation

processes. (5) Collapsing large globules (Barnard objects), of low temperature and chockerblock-full with CO (and thus with molecular hydrogen), provide a good basis for the building of less massive stars; two Herbig-Haro Objects have been found associated with a southern globule (see *Publ. Astron. Soc. Pacific*, October, 1978). Professor Reddish alludes vaguely to most of these processes, which were, however, not generally recognised as being as basically important in 1974-75 as they are today. I wish we might persuade him to add an additional volume to his present offering and bring us up to date.

The second half of the book presents the more theoretical aspects of star formation. It is an excellent and comprehensive treatment but it too is dated. He shows nicely that the physical properties of interstellar gas and dust are such that an interstellar cloud structure is inevitable. Chapter 9 is a most readable presentation of the Lin-Shu density wave theory of spiral formation and its effect on interstellar cloud evolution. There follow very fine chapters on cloud contraction, especially on fragmentation and its effects on the distribution of protostar masses. The chapter on the rate of star formation is regrettably brief and the concluding chapter on the evolution of galaxies did not stir this reviewer.

The book is nicely produced and the medium-priced paperback edition handles well. There are a few places in which a little more care in proof-reading would have been helpful; I noted that the word "asymmetry" is consistently misspelled (pp153, 164, 165). On page 171, there is a reference to (Roberts, 1969); one finds in the bibliography two such references—one to M. S. Roberts, the other to W. W. Roberts—the second one being the one that is indicated.

Because of its underlying unified personal approach of Professor Reddish, this is a book that should be read by everyone who works in the field of star formation. Once one accepts the obvious limitations, we can all learn from his treatment of star formation. This is a good book to assign to one's students, provided it is supplemented by the other volumes mentioned in my review; it is a good treatment for the beginning graduate student, and it should also appeal to advanced undergraduate students in the physical sciences. Professional physicists will find that this book has much to offer, as will informed amateur astronomers. □

Bart J. Bok is Emeritus Professor of Astronomy at the Steward Observatory, University of Arizona, Tucson, Arizona.

Stellar evolution

R. J. Tayler

Stellar Evolution. Second edition. By A. J. Meadows. Pp. 171. (Pergamon: Oxford, 1978.) Hardback £7.50; paperback £2.50.

THE subject of stellar structure and evolution is believed to be the branch of astrophysics which is best understood. Because most parts of physics—such as nuclear physics, atomic physics, thermodynamics and statistical physics, electromagnetism and gravitation—are involved in determining the structure of a star, it is a very suitable topic in applied physics to introduce to undergraduates. It is also possible to present the subject to an audience with a slighter knowledge of physics and this is the concern of Professor Meadows.

When the first edition of this book was published in 1967, it received a warm welcome because of the manner in which it bridged the gap between very superficial discussions of stellar structure and evolution in books for the layman and complex mathematical texts for the professional astronomer. Twenty years earlier George Gamow's book *The Birth and Death of the Sun* had served a similar purpose, but by 1967 this was seriously out of date in detail. The first edition of Meadows' book gave a clear account of the observed properties of stars and the manner in which they are obtained. It then discussed the principles underlying the structure of stars, the source of the energy radiated from stellar surfaces, the manner in which this energy travels from the centre to the surface of a star, and the physical state of the matter inside stars. The remainder of the book was concerned with the evolution of stars from their birth as dense condensations in interstellar gas clouds, through the major part of their life as main sequence stars (the present state of the Sun) and subsequently as red giants, to their death as white dwarfs, possibly after a supernova explosion. It was then difficult to see how our knowledge of stellar evolution could be treated in a significantly clearer manner without the introduction of mathematical detail, which was completely absent from Meadows' book.

The first edition has now been revised to take account of developments in the subject. Most of the book has been photographically reproduced from the first edition and in most places it is possible to see where changes have been made because of a slight variation in

contrast of the type. In the first half of the book there are only a small number of detailed alterations but later there are much more substantial changes, particularly in chapters 6, 7 and 8. The book is essentially unchanged in length, the addition of new material being balanced by the removal of speculative or unimportant material.

Substantial changes in the new edition include the following. A section has been added on the solar neutrino experiment, the results of which are at present the major question mark against the standard view of stellar structure. There is a completely revised discussion of the evolution of low mass stars following the helium flash, the explosive onset of the conversion of helium into carbon. The major change concerns stellar old age and death. Shortly after the first edition appeared, pulsars were discovered and it was

soon realised that they are neutron stars. It now seems certain that neutron stars rather than white dwarfs are a normal end-product of a supernova explosion. In addition it has been realised that sufficiently massive stellar remnants will be neither white dwarfs nor neutron stars but will be black holes. Rather surprisingly in view of the great current interest in close binary stars and mass exchange between their components, the section on this topic has been slightly reduced in length, although there is a brief mention of the possible detection of a black hole in Cygnus X-1.

The revised edition of this book remains a very good non-mathematical introduction to stellar evolution and the paperback edition is very reasonably priced. □

R. J. Tayler is Professor of Astronomy at the University of Sussex, Brighton, UK.

Microcosm of astrophysics

F. Graham Smith

The Crab Nebula. By Simon Mitton. Pp. 194. (Faber and Faber: London; Scribners: Totowa, New Jersey, 1979.) £6.50; \$14.95.

THE 900th birthday of the Crab Nebula was celebrated by radio astronomers in July 1954. At that time it was almost the only celestial object, apart from the Sun, where a close link between optical and radio astronomy had emerged. In these days, when such links have appeared throughout astronomy, the Crab still has a unique importance, and annual celebrations of the supernova explosion would still be appropriate. Geoffrey Burbidge once said that astronomy could be divided into two parts: the astronomy of the Crab Nebula and the astronomy of everything else. There are indeed links between the Crab and almost everything else in astronomy, so that a discourse on the Crab can be used as an introduction, for example, to astronomy, stellar evolution, cosmology and interstellar matter; including the Crab Pulsar extends the list to the physics of condensed matter, high energy electrodynamics, superconductivity, and many other aspects of quantum physics.

This microcosm of astrophysics can therefore be used as a general introduction to astronomy, which should appeal to any scientist and which may persuade some that astronomy is a very useful means of education in basic physics. Popular books on astronomy

often do not succeed in presenting the more serious aspects of the subject, and their readers might not realise the important part which is played by astronomy in postgraduate physics education. Mitton is a physicist, and emphasises the basic physics throughout, but his book is nevertheless a popular book. He eschews mathematics, and uses not a single equation. The reader need only be moderately numerate, so that he can appreciate the large powers of ten involved in any branch of astrophysics.

The decision to exclude mathematics reduces the astronomy of the Crab to less than half of the total of astronomy; and further choices have been made. Mitton has chosen to make the book readable, straight through, for anyone with an undergraduate level of physics or astronomy. Others, with less expert knowledge, will enjoy at least the historical accounts of the supernova explosion, gleaned from Chinese and Japanese records of the "guest star", and the more recent history of discoveries—notably of the pulsar which remains the centre of the nebula, providing the powerhouse for the energy to keep the nebular shining. For the physicist, the exposition of the basic concepts of degeneracy in white dwarfs and neutron stars, and of superconductivity, will have a strong appeal, as they show most clearly the connection between terrestrial and celestial solid-state physics.

Mitton is an enthusiast. In conveying his enthusiasm he does sometimes overstep the mark, in such purple passages as his description of synchrotron radiation: "photon screeches from very energetic electrons as they wrap themselves round the ambient magnetic field". He refers to the giant pulses from the pulsar as "jumbo", he tells us

that the "youngest pulsars are the jazziest". He also obtrudes a view on space research: the X-ray observations were "made at truly vast public expense". Colourful presentation, but nothing incorrect. Colour is a strong advantage in places—for example, where the energy of one mosquito is given as a million million electron volts.

The astrophysics is sufficiently complete and correct that one can regret quite small omissions, such as the optical polarisation of the pulsar, and

wish to dispute the conclusions on the birthrate of neutron stars in the Galaxy. The serious reader is encouraged to consult a useful but rather short bibliography. This will certainly not be adequate for astrophysicists, and the index is also far too short. Nevertheless, the book is strongly recommended. □

F. Graham Smith is Director of the Royal Greenwich Observatory, and Visiting Professor of Astronomy at the University of Sussex, Brighton, UK.

Anatomy of a stellar murder

Donald D. Clayton

Stars: Their Birth, Life and Death. By I. S. Shklovskii. Pp. 442. (Freeman: San Francisco and London, 1978.) £10.70.

ARE you a practising astrophysicist unable to keep up with the literature, too fatigued by your own specialty? Are you worried that you are becoming too narrow while an exciting flood of astrophysical discovery flows all about you? Then this book is for you. Or are you a physicist wondering what the physics of modern astronomy is all about? This book is for you too. It is also for the amateur astronomer or the student who wants to hear the real issues of astrophysics from one who wrestles with them and who can also describe them without needless complication on the one hand or sterilising simplifications on the other.

Iosif Shklovskii writes incisively about the full range of what I would call stellar astrophysics. Not addressing extragalactic astronomy or cosmology on the one extreme, nor the origin and evolution of the Solar System at the other, Shklovskii concentrates on "stars, the most interesting and important objects in nature." But what stars! Not the serenely twinkling objects we see above us, but rather the collapsing, masering, exploding, bouncing, pulsing, bursting, spinning, extincting labour of birth and violence of death in the heavens. What makes his "serious popular book" (in the author's own words) so exciting is the closeness of many thrilling discoveries to Shklovskii's own career. His brilliance as a thinker and writer is evident throughout, not at all dampened by what must be a very good translation from the Russian, in which language the book was first published in 1975. This English edition has been updated by the author

to render it even more current, although currency is not the main objective.

Perhaps the book should have been called *Anatomy of a Stellar Murder*, so vividly described are the clues marking the birth, ageing, and death throes of its central character. The reader feels like a detective on the case. The book unashamedly emphasises the newer areas of astrophysics, especially the evidence of birth in molecular clouds and the phenomena associated with neutron stars. Shklovskii correctly credits radio astronomy with the leading role in these discoveries, perhaps somewhat under-representing the contemporary excitement in optical astronomy because it is older and, alas, just as taken for granted as a wife of several decades. It would have been Shklovskian to have paused to conjecture how

different it would all have been had we by accident evolved within an optically opaque atmosphere.

This book is quantitative, but not mathematical, displaying only important equations. A strong point is the presence of the many clarifying figures, which seem almost as abundant as the equations. Not designed as a textbook it would nonetheless fascinate final-year astronomy undergraduates or beginning research students, and could be supplemented by physics derivations in class. I would use it for such purposes without hesitation. In fact, the publisher should for this market consider issuing a separate companion paperback of derivations and problems at this level. This is, however, a personal book with the author as its main attraction. It has no references, except a general list at the end, but Shklovskii is liberal with text credits to pioneering researchers. The fact that half of these salutes are to Russians and to himself seems, in this case, justifiable by the author's own major role and by a general lack of awareness in the US of research in Russia.

The book is in four parts, having a total of 24 chapters. Some of the chapters, especially those on Cosmic Masers, Evolution of Protostars, the Crab Nebula, Neutron Stars and Pulsars, and X-ray Stars, are so fascinatingly written as to be pinnacles of astronomical exposition. Don't tell me that when it comes to writing serious popular books there is no difference between a great thinker and merely a great writer. Shklovskii is in the former league with Fred Hoyle and Carl Sagan. Only in the chapters on normal stars does Shklovskii lose the torch. In discussing white dwarves, for example, he seems to slip from his role as excited guide to that of teacher, beginning to wax pedagogical. Stellar structure and evolution suffers also in comparison, results of calculations tending to supplant discussions of physical mechanisms. Nucleosynthesis in stars is not described at all. A few controversial positions are taken up, such as that the admixture of overlying ³He excesses is the resolution of the solar neutrino problem. But maybe it is only in the eyes of this reviewer that this part of the book seems less stimulating than the mystery of the death screams of the heavenly moribund.

All in all, this is a wonderful addition to the astronomical literature, not only as an introduction to modern issues in stellar astrophysics but also as a chronicle of the way of thinking and the temperament of one of its most colourful practitioners. □

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Origin of the Solar System

Fred L. Whipple

The Cosmogony of the Solar System. By Fred Hoyle. Pp. 168. (University College Cardiff Press/ Christopher Davies Publishers: Swansea, UK, 1979.) £7.95.

THIS book is not for the general public nor even for the serious amateur astronomer. The physics and the mathematical equations are critical to much of the discussion and, even though painstakingly explained, remain high hurdles for readers without advanced training in these subjects. On the other hand, Sir Fred writes down to research students and to his scientific peers. Had he published the book a decade or so ago as a scientific monograph with proper referencing to the host of contributors to the field, it would have been a useful summary of Sir Fred's imaginative thinking on the subject. It could have stimulated progress in understanding the complex processes that combined to form the Solar System and helped explain how we came into being. Today *The Cosmogony of the Solar System* serves chiefly as an historical document which can be seen to illustrate the formidable intricacy of the problem as measured in masses of data, in diverse physical processes and in time sequences of events.

Nearly everyone agrees on the gross simple picture. Collapse within a black interstellar cloud of dust and gas formed the Sun, the planets, the asteroids and the comets. Since 1950, when the comets were recognised as dirty ice balls, it has been apparent that temperature was the controlling factor in the evolution of the Solar System. The temperature decreased with increasing solar distance. Near the Sun only rocky material could freeze, so that within Jupiter's orbit dust aggregated to form planetesimals and the rocky terrestrial planets. Beyond Saturn icy cometesimals were the building blocks that formed Uranus, Neptune, probably Pluto, and some of the satellites of the great planets. Jupiter and the Sun are considered by some investigators as a double star system and in a certain sense Sir Fred agrees. This all happened 4.6 Myr ago. The details remain obscure and therein lie the controversies. Some of these complications are elaborated in *The Cosmogony of the Solar System*.

Sir Fred makes the basic assumption that the black cloud collapsed to the

radius of Mercury's present orbit, then expanded as a rotating disk star to the limits of the planetary system. His concept fails in at least three major respects, each of which is well documented in the literature and each of which negates most of his thesis. First, a collapsing cloud in the interstellar medium cannot collapse as a simple entity. Nature really does abhor a vacuum; the collapse brought with it an influx of outlying gas and dust, horribly complicating the situation. Secondly, Sir Fred utilises "magnetic brushes" from the rapidly rotating central mass to expand the dust. He assumes that the expanding gas will carry along solid particles that condense as the temperature falls with distance. In fact, the gas in the disk carries an outward radial pressure that reduces the effective central gravity field. The solid particles feel only the gravity field and try to revolve faster than the gas. Thus, the gas acts as a resisting medium causing the particles to spiral inward. No rocky material, therefore, should progress beyond Jupiter's orbit, contrary to the evidence from comets and probably Uranus, Neptune and some outer satellites. Thirdly, Sir Fred adopts the "minimum mass" concept which assumes that the only matter "wasted" from the system is gas that could not condense into the planets, asteroids and comets. The other strongest proponents of the "minimum mass" concept (to whom he does not refer) have long since abandoned the

idea. Also Sir Fred's ingenious removal process—interaction with interstellar clouds—is marginally possible at best. It fails completely if nature is really as wasteful as most investigators now believe.

The accounts of the aggregation of condensates into planets are lucid and Sir Fred presents clearly the problems of explaining rotations. His thinking here follows currently conventional patterns except for Jupiter and Saturn. Great uncertainty, however, remains as to the processes which formed these giant planets. Some of the geophysical presentations will surely raise eyebrows and comments among specialists, particularly the concept that radioactive elements such as potassium, uranium and thorium were concentrated in the upper mantle of the Earth as a late time-sequence layering process rather than by magmatic differentiation. Related concepts of lunar evolution and structure are also novel and highly controversial.

As for Sir Fred's proposal that life originated in comets, I am charmed but not impressed by the picture of life forms developing in "warm little ponds," protected in their icy igloos from the cruel cold and near vacuum of open space and falling to primitive Earth at speeds exceeding eleven kilometres per second. □

Fred L. Whipple is Phillips Professor of Astronomy Emeritus at Harvard University, and Director Emeritus of the Smithsonian Astrophysical Observatory, Cambridge, Massachusetts.

Heroes on the climatic stage

W. S. Broecker

Ice Ages: Solving the Mystery. By J. Imbrie and K. Palmer Imbrie. Pp. 224. (Enslow: Short Hills, New Jersey; Macmillan: London, 1979.) \$12.95; £6.95.

ANY hit parade of prominent problems in the Earth sciences would have to include among its top ten the cause of the glaciations which have plagued the Pleistocene. Despite the prominence of this problem relatively little has been written about the history of the endeavour to explain the instability in our planet's climate. The Imbries have not only plucked this plum but have done a superb job. They provide a very readable insight into not only the development of scientific thinking on this subject but also into the person-

alities of the key participants.

It is a book of heroes. Croll, from the Imbries' ancestral Scotland, receives top billing for his role in advancing the orbital theory. Milankovitch, with whose name the orbital theory is



James Croll

normally linked, is portrayed as a patient and confident mathematician who devotes his life to calculating the geographical and temporal variations in monthly isolation. John Imbrie's fellow detectives in the current investigation aimed at tracking down evidence in support of the orbital theory are given strong supporting roles.

The Imbries' account shows how personality conflict and serendipity complicate the path towards scientific discovery. Rarely does the key evidence emerge from a carefully planned strategy. More often it emerges from arguments between parties holding divergent views or from measurements made for some other purpose. Perhaps one of the greatest contributions of books like this is to demonstrate to the public the necessity of supporting basic research. The carefully designed pert charts of government agencies rarely produce major discoveries. Creativity cannot be programmed.

The Imbries' account shows the incredible breadth of evidence and technique which have contributed to our understanding of climatic history. The stage is successively dominated by the field geologist observing grooves and erratics, the oceanographer looking

down the microscope at microfossils from deep sea cores, the isotope geochemist with his mass spectrometers and radioactivity detection systems, by the paleomagnetician, and yet again by the field geologist and by the micropaleontologist. Interwoven with field observation are mathematical calculations involving celestial mechanics, isolation dynamics, factor analyses and spectral analyses.

The Imbries are evangelists actively involved in the revival of a once prominent and then much maligned hypothesis. Although they wish the reader to believe that like continental drift the Milankovitch hypothesis is being swept back into fashion by a landslide of new evidence, this reviewer suspects that the CLIMAP *blitzkrieg* will be slowed by a wave of resistance. Like Agassiz, Imbrie and his colleagues will encounter the Bucklands and Lyells of modern meteorology. They are steeped in the dogma that the orbital changes are far too weak to be responsible for the awesome changes from a glacial to an interglacial world. Thus, rather than constituting the final report on solution of a major scientific problem, the Imbries' book will more likely prove to be a status report on a continuing investigation. Many new heroes will likely occupy the climatic stage before the mystery is solved.

The Imbries' enthusiasm for the isolation hypothesis in no way detracts, rather, it gives the book an excitement which allows it to transcend most treatments of the history of science. Their book does for climate what J. D. Watson's *The Double Helix* did for



Milan Milankovitch in 1943

Portrait by Paja Jovanovic

molecular biology. It allows the reader for a few hours to participate in the intrigues, the mystery, and the excitement of scientific discovery. I heartily recommend this book not only to my fellow Earth scientists but also to their wives, children, neighbours and acquaintances who find it difficult to comprehend our preoccupation with the past. □

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Louis Agassiz at the Unteraar Glacier

Portrait by Alfred Berthoud in the University of Neuchâtel

Scientific journalese

Joe Schwartz

Experiencing Science. By Jeremy Bernstein. Pp. 275. (Basic Books: New York; Burnett Books/André Deutsch: London, 1978.) £5.95.

JEREMY BERNSTEIN could be called the C. P. Snow of the 1960s. Writing with an authority and confidence characteristic of Snow's novels Bernstein communicated a literate insiders view of the high energy physics community during the easy money days of ten years ago. The present collection of articles, published from 1969 to 1978 reflects the strains induced in science by the financial stringencies of the seventies and a different social and political cli-

mate. Bernstein begins to show signs of struggling with the changed circumstances, but the results are not as satisfying as his earlier work.

His evaluation of Nora Sayre's book on Rosalind Franklin suffers from an insensitivity to the very real difficulties women experience in a male dominated profession. By concentrating on the facts of the case as presented in Robert Olby's book, *The Path to the Double Helix*, Bernstein chooses to avoid the underlying issues of patriarchy and male domination that prompted Ms Sayre to write her book in the first place. This attitude intervenes to weaken an interesting attempt to describe Bernstein's personal struggle to understand Goedel's Theorem (there exist true mathematical statements that are unprovable). Bernstein introduces a lady friend, K., with whom he will discuss his difficulties. Unfortunately all she does is cook him a meal, listen to a lecture and go to bed. She has no lines of her own. K.

could have been a mathematician and Bernstein the listener which would have given him a chance to show more directly where his understanding breaks down. Instead we end up hearing a lecture rather than seeing the kind of effort it takes to understand the complexities of metamathematics.

But the long piece on US physicist I. I. Rabi is a gem. This is Bernstein at his best. We have a fascinating story of the shift of the centre of gravity of physics research from Europe to the US in the inter-War years as seen through the eyes of Rabi, a Nobel Prizewinner in 1944 for work on molecular beams. Bernstein succeeds in presenting a fresh account of the most romantic period in the history of physics. Here is Rabi criticising the

present division of labour between theorists and experimentalists in physics: "You do an experiment because your own philosophy makes you want to know the result. It's too hard, and life is too short, to spend your life doing something because someone else has said it's important. You must feel the thing yourself—feel that it will change your outlook and your way of life. You must bring it back to the human condition, the human expression—much closer to what the artist is supposed to feel." In the present period there are not many physicists who can experience this kind of emotional commitment to their research. □

Joe Schwartz is on leave from the City University of New York.

Gravitation and the expansion of the Earth

T. C. Van Flandern

Cosmology and Geophysics. By P. S. Wesson. Pp. 240. (Adam Hilger: Bristol, UK, 1978.) £17.50.

It is not immediately evident that the book's two subjects, cosmology and geophysics, have very much to do with each other. The primary connection is through expansion of the Earth, a predicted consequence of several recently proposed cosmological theories and a favourite theme of the author. If the Earth does indeed expand with time, this provides a driving mechanism for continental drift, a simple understanding of how the Earth's surface became partitioned into continents and ocean basins, and in general a deeper understanding of the origin and evolution of the surface features of planetary bodies. The evidence that the Earth has, in fact, been expanding in the past is still debated; but the author presents a credible case for his interpretation. The evidence ranges from the suggestion that all of the continents would fit together nicely on a globe having one-fourth the Earth's present surface area, to palaeomagnetic determinations of the Permian radius, and arguments from logic: How could Antarctica become almost surrounded by an ocean ridge, with which it was once supposedly in contact, unless the globe has expanded?

Two mechanisms for producing the

expansion are offered, and this leads us immediately into cosmology. The first is that the universal gravitational constant may be decreasing; the second is an expansion of all macroscopic bodies and the space they occupy relative to the size of the atomic particles which comprise them, at the same rate as the observed large scale expansion of the Universe. These two mechanisms can now be unified into a single theory, which has been the direction that most theoretical work has taken since this book was written.

The book opens with an excellent survey of variable-*G* cosmologies and what motivated them; Milne, Dirac, Jordan, Gilbert, Brans and Dicke, and Hoyle and Narlikar are all reviewed. There follows a general discussion of post-Einstein gravitational theories, of the various attempts to incorporate Mach's Principle of Inertia into these theories, of space variability of the gravitational constant, vacuum elasticity, possible anisotropy of gravitation and the limits placed by the Eötvös experiment, and additional related topics. There is then an entire chapter devoted to the Eddington numbers, which form the basis of the Large Numbers Hypothesis and the motivation for the original 1937 proposal of Dirac that gravitation must be weakening with time. This chapter also discusses experimental limits on the variations of other fundamental quantities, such as the fine structure constant, and discusses the physical and astrophysical consequences of the proposed changes in fundamental constants of physics.

This summarises the first half of the book. It is a truly excellent review with a very complete bibliography. Moreover I was pleased to see explicit mention of the assumption underlying this entire discussion, that there may be as yet undiscovered laws of physics. This

has always seemed to me an obvious point as long as our understanding of the true nature of phenomena such as gravitation is so limited (Why do masses attract only? Is there a quantum of gravitation? Is shielding possible?); yet it is an assumption few physicists seem to make. The only criticism to this point is that reading would not be easy going for the novice, as specialised terms are sometimes introduced with only very sketchy definitions and no real attempt to provide understanding of the concepts. There is also confusion on p42 about just what information ancient eclipses provide.

The second half of the book deals with experimental evidence on changes in gravitation and Earth expansion, and an attempted reconciliation of such concepts with Relativity Theory. The author very much favours the idea that the Earth has nearly doubled its radius in its lifetime, rather than the more modest expansion of perhaps 10% allowed by a weakening of gravity. I think he has overlooked a valuable argument. We know that the Moon must have been quite close to the Earth in the past; the best estimates place the closest approach at about 3×10^9 yr ago. Tidal friction under these circumstances must have been enormous, and surely must have produced considerable internal heating and expansion of the Earth. Moreover, if the Earth had a much smaller radius than at present for most of this early period, this would help explain why the tidal coupling coefficient has averaged less than half its present value since the Moon was close. Whether the expansion continues today would then depend on whether gravitation is weakening; but the bulk of the expansion would have taken place early in the Earth's history. Moreover, it is important to keep in mind that expansion from any cause would surely occur in discrete events rather than continuously, as radial strain, like horizontal strain, would have to reach a threshold before the Earth would yield. It is interesting to note the recent discovery of a huge surface fissure on Venus, comparable to the largest known on Mars. Taken with the large Van de Graff lunar farside surface depression, it is probably not premature to infer that expansion may indeed be a regular feature of planetary evolution. If so, Wesson deserves much of the credit for advocating the idea while it was still unpopular, and for placing it in a reasonable theoretical and observational perspective. □

T. C. Van Flandern is Chief of the Celestial Mechanics Branch of the Nautical Almanac Office, US Naval Observatory, Washington, DC.

Key to the mysteries

John M. Charap

Einstein's Universe. By N. Calder. Pp.147. (Viking: New York; BBC Publications: London, 1979.) \$10; £6.25.

SOME of the very best popularisations of science to have been published in the past decade have had their origin in BBC television productions: these have included Nigel Calder's own *Violent Universe* and *The Key to the Universe*. And now comes, in celebration of the centenary of Albert Einstein, *Einstein's Universe*. As in his earlier books, Mr Calder has drawn upon the resources deployed by the BBC in their original telecast form and has made a brave effort to meet the challenge inherent in explaining the intellectual achievements of fundamental physics without either scandalising the experts, boring the lay readers for whom he writes, or still worse misleading them. That there is a very significant desire for information, a craving for a simple route to understanding, a key to the mysteries, cannot be doubted. Any physicist who has been button-holed at a cocktail party and asked to 'explain' quasars or lasers, quarks or black holes, can testify to that. But he can also give evidence of the difficulties: how to explain modern physics to an intelligent, inquisitive but non-numerate questioner whose understanding of classical physics is likely to be non-existent. It's not just making the progression from nineteenth-century science to quantum physics and relativity, but rather going from Aristotle to Einstein leapfrogging Newton.

The millions who watched the television broadcasts may have been satisfactorily distracted by the glamour of the hardware, or entertained by Peter Ustinov's German accent, or fascinated by the contrasting personalities of the contributing scientists. And in his earlier book Mr Calder allowed himself some glamorous packaging, beautiful colour photographs, coffee-table format. This time his approach is more austere: indeed he seems to have taken his task to "make relativity plain" almost literally and *Einstein's Universe* has few illustrations, none in colour. I would even question whether the photographs chosen actually illustrate anything very significant to the purpose of the book, but am grateful for two of Einstein

himself, neither of them over-familiar. Einstein thought visually, as do many creative scientists, and the book would have been better, and the author's task easier, if there had been more and better diagrams. One of the few which actually relate directly to the text (p47) is annoyingly wrong, and even if it were corrected it would still be less informative than might have been possible with better draftsmanship and captioning.

The sequence of topics chosen by Mr Calder to lead the reader right up to today's research is unusual and I think effective. Instead of the conventional historical approach, from special relativity to general, the order is reversed. Moving swiftly from the transverse Doppler effect to $E=mc^2$ and then to gravitation, the basic

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themes are blocked in. At the same time opportunity is taken whenever it is presented to make reference to the most recent experimental or observational confirmation of the consequences of the ideas. So it is that very early in the book the evidence from astronomy for the existence of black holes, as collapsed stars (Cygnus X-1) or as galactic nuclei (M 87) is woven into the argument. Einstein's contributions in fields other than relativity are also brought in to further the main development of ideas. So his notion of stimulated emission leads through atomic clocks to the gravitational effect on clocks and on time. The 'time-shells', successive zones in which clocks run at greater rates as one goes further from a massive object are skilfully introduced and lead without strain to the gravitational red-shift.

The equivalence principle, so central

to Einstein's own way of thinking is ably introduced in a chapter on weightlessness: the advantage an author who can draw on the reader's experience, if only vicarious, of free-fall in a space-capsule is very substantial and is well-exploited.

The effect of gravity on time is then used as a bridge to the effect of gravity on space. The over-worked, and to my mind misleading analogy with the distortion of a rubber sheet by heavy objects is used, but more telling is the difference in surface area of a sphere enclosing a mass from the $4\pi r^2$ of Euclidean geometry—the surface of the Sun has a 'missing' area roughly the area of the USA.

The famous classic tests of general relativity, and their modern counterparts are all described; not just the perihelion advance of Mercury, but the periastron advance of the binary Pulsar 1913+16. The book went to press when it was only possible to speculate that that same binary pulsar might give evidence through Joseph Taylor's careful and continuing observations for gravitational radiation; that confirmation was announced last December. The deflection of star-light by the Sun observed by Eddington's 1919 expedition is explained, and the counterpart annual observation of the same effect on 3C 279 also described. The so-called 'fourth classic test', the radar echo delay, is here too.

I think the treatment of the observational consequences of general relativity and the present status of such observations, is very good indeed. I'm less happy, however, with the discussion of special relativity, which is often made less than clear by the author's attempts to verbalise the mathematics. However, the reasoning for the apparent rotation of a fast-moving object (and hence its foreshortening) is really very well handled, and overall I think it fair to say that many much fuller accounts of special relativity, in popular or technical treatments, succeed far less ably in getting the basic points across.

The book's last main theme is an exploration of relativistic cosmology; inevitably and quite properly this leads into more speculative areas. In the course of the book we have already had our attention directed to quantum effects, to Hawking's quantum radiance of black holes, to gravitons (there's one sentence I shall try to remember for its explanation of the non-linear effects of quantum gravity: "This incest among the gravitons produces the curvature of space"). And now we meet supergravity and gravitinos, twistors and foamy space.

I enjoyed reading the book. I was occasionally irritated by infelicities like "At its birth light accelerates infinitely

Einstein in 1916

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rapidly" or "Even more fundamental than the speed of light is energy". (What exactly can that mean?). More serious a criticism is the worry that in spite of his best efforts the reader may fail to grasp the logic of the theory rather than be convinced by evidence in its favour. But Mr Calder has made

a valuable contribution to the demystification of relativity, and one which is most worthy to appear in this year of Einstein's centenary. □

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Einstein's charisma

W. B. Bonnor

Einstein: A Centenary Volume. Edited by A. P. French. Pp. 332. (Heinemann: London; Harvard University Press: Cambridge, Massachusetts, 1979.) £10.50; \$25.

THE personality of an intellectual giant like Einstein exerts special fascination. How far is his genius apparent in everyday behaviour? Does he have special insight into non-scientific matters? How does he get on with his ordinary fellows? It is fortunate from this point of view that Einstein's life and opinions have been so well documented: indeed so much has been written about him that it is quite hard to say anything new.

Einstein: A Centenary Volume was written at the instigation of the International Commission on Physics Education, bearing especially in mind teachers and students of physics at secondary and undergraduate levels. It consists of four parts. Part I contains 18 reminiscences and reflections on Einstein, half of them reprinted from elsewhere. Part II is the main part of the book containing thirteen articles on Einstein's scientific work and political and social ideas. Twelve of these seem to be new, and specially commissioned for the book. Part III contains a number of letters from and to Einstein: it is not made clear which (if any) of these are being published for the first time. In Part IV a number of Einstein's writings are reprinted and a pitifully inadequate bibliography is given. The book contains many interesting photographs and drawings and is lavishly produced. However, it certainly does not offer a rounded account of Einstein's work or his personality, and the statement on the dust-cover that the book is "a total assessment of the life and works of one of the intellectual giants of our age" is unjustified. To mention one important omission, there is no description of

Einstein's work on cosmology.

In the commissioned essays there are several worthy of serious attention. Silvio Bergia gives a scholarly history of the discovery of special relativity, starting from the relativity of Newtonian physics. His article contains a careful judgement of the parts played in the discovery by Lorentz, Poincaré and Einstein, with special reference to the assessment of E. T. Whittaker, who called special relativity "the theory of Poincaré and Lorentz". Bergia concludes that Whittaker's view cannot be sustained, and that the credit usually given to Einstein is justified. There are competent accounts of Einstein and the quantum theory by Martin J. Klein, and of the observational verification of general relativity by A. P. French.

The highlight of the book for me was the article, "Relativity theory and gravitation", by Hermann Bondi. Bondi presents a derivation of general relativity which differs from that of Einstein, and which he attributes to Fock. He starts with the notion of gravitation as a tidal force only, so that a uniform gravitational field is a zero gravitational field; and he discusses the nature of mass, describing the three different sorts and commenting on the non-existence of negative mass (of the reason for which relativity gives no clue). A somewhat

sophisticated treatment of Newtonian gravitation theory is set up for later generalisation. Gravitation is introduced into special relativity by a thought-experiment using the gravitational red shift. A simple introduction to non-euclidean geometry leads painlessly to the Riemann tensor and the equation of geodesic deviation, which is neatly tied up with the Newtonian treatment and the tidal approach. It is beautifully done and should be read by all those interested in general relativity. One feels that Einstein, with his eagerness to explain relativity to the man in the street, would have enjoyed this article.

Among the essays on social and political matters are "Einstein and world affairs" by A. P. French and "Einstein and Zionism" by Gerald E. Tauber. Here Einstein emerges as the idealist, working to put the world right, but almost as much at sea as the rest of us. He espouses utopian ideas such as world government, disarmament and democratic communism; he writes to Roosevelt in 1939 calling attention to the possibility of building an atomic bomb, but later he is horrified when one is dropped; he writes to the President of the USSR Academy of Sciences asking for contributions to a book *One World or None*, and gets a dusty answer. All this was well meant, of course, but how naive and ineffective compared with the majesty of his achievements in physics!

In this field there emerges too a side to Einstein somewhat at variance with the saintliness which his reputation usually enjoys. Although he wrote that he passionately hated "all the loathsome nonsense that goes by the name of patriotism", yet, unable to detach himself emotionally from his Jewish origin, he became a Zionist. He went on fund-raising tours and let himself "be exhibited like a prize ox". Even before the Nazi era, Einstein disliked the Germans and after World War II he developed a pathological hatred of them, calling them mass-murderers. This attitude contrasts ill with that of his Jewish colleague Max Born who showed a gentle forgiveness for Germany after World War II.

Although, as we ought to expect, Einstein had his weaknesses, the book brings out many of his fine qualities. One is his modesty and unpretentiousness—illustrated by the story of how, at a dinner in his honour at which a too fulsome speech was being made about him, he whispered to his neighbour, referring to himself, "But he doesn't wear any socks!" □

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Special relativity

Bernard F. Schutz

Special Relativity: The Foundation of Macroscopic Physics. By W. G. Dixon. Pp. 261. (Cambridge University Press: Cambridge and London, 1978.) £14.50.

DON'T let the title deceive you: this is not another introduction to special relativity. This is a book for those of us, working physicists or advanced students, who know a few things about relativity, thermodynamics, and electrodynamics but have not bothered to examine too closely how all these things fit together and on what sorts of assumptions they are based. Graham Dixon's aim is to isolate these assumptions and to derive from them the macroscopic laws we know so well, making full use of the spacetime viewpoint. On the whole he succeeds very well, and the book should prove rewarding to the careful and patient reader.

The first chapter studies the foundations of relativity (special and Galilean). Dixon discards the usual postulates of relativity (that the laws of physics are the same in all frames) and of the invariance of the speed of light (or for Galilean relativity, the usual velocity-addition law) in favour of two other postulates. One is the Principle of Uniformity, that in a given frame, space and time are homogeneous and space is isotropic. The other is the Extended Principle of Inertia, which not only extends the usual principle of uniform motion in the absence of forces to macroscopic bodies but also postulates that a clock's rate may depend on its speed but not on its position or direction of motion. From these two principles and a careful discussion of the nature of measurements, Dixon arrives essentially at the conclusion that the transformation law between the preferred reference frames picked out by the principles must be linear and must preserve the line element $(\delta x)^2 + (\delta y)^2 + (\delta z)^2 - C(\delta t)^2$, for some constant C . The case $C > 0$ is special relativity, and $C < 0$ is excluded by a rather weak argument which would have been unconvincing before the days of particle accelerators. The limiting case, $|C| \rightarrow \infty$, is actually treated separately and is, of course, Newtonian theory. (The case $C=0$ is not discussed but is in fact easy to exclude.) Later Dixon remarks that it seems as if we have got something—a fundamental constant with the dimensions of velocity—from postulates

which seem to have no scale, and that this velocity may originate in the fundamental velocity $\hbar/e^2$ by way of quantum-mechanical nature of the measuring rods one uses. Although there may be something in this, a more direct explanation seems to me to be that his postulates already imply the consistency of using universal units for time and distance, and the only question is what value C has in these units. As the postulates do not restrict C , they do not themselves give a universal velocity. This restriction comes from outside the postulates, by adding that the cases $C < 0$, $C=0$, and $|C|=\infty$ are contrary to experiment. But this is a minor quarrel, which should not obscure the fact that this chapter makes the useful point that special relativity and Newtonian physics need not be derived from such disparate postulates as one usually finds in elementary treatments.

This unity of treatment of relativistic and Newtonian theories is developed further in chapter 2 and then carried through the whole book. Chapter 2 begins by developing affine tensor calculus. The point of view is algebraic rather than geometric, with tensors defined by their transformation properties rather than as linear functions of vectors. Integration on arbitrary surfaces in an affine space E_n is discussed, with a full and clear treatment of antisymmetric tensors, volume elements, orientation and Stokes' theorem. Lie derivatives don't appear here but are introduced very briefly in chapter 4. In order to treat relativity and Newtonian theory in a unified manner, their respective spacetimes are discussed and fundamental tensors defined: the metric for relativity and both a 3-metric and the universal time covector in Newtonian theory. (Throughout the book Dixon uses 'Newtonian' to mean a gravity-free spacetime, which others often call Galilean.) A particularly nice feature is a demonstration of how to contract the Lorentz group to get the Galilean group.

There follows a discussion of particle and continuum dynamics, conservation laws, and an especially clear discussion of the effects of forces on mass and energy in both theories. One can also find here in chapter 3 a careful treatment of the 'Newtonian limit', which deserves a careful treatment where mass and energy are concerned.

In chapter 4, Dixon picks up his assertion that the laws of physics are seen most simply from a relativistic point of view by showing how much of thermodynamics and phenomenological hydrodynamics can be derived from exceedingly simple assumptions. His starting point is the entropy-flux four-vector s^α , which by the second law of

thermodynamics satisfies $s^\alpha_{;\alpha} \geq 0$, the equality obtaining in equilibrium (but not only there). He defines a simple fluid as one in which s^α is a function only of the stress-energy tensor and the particle-flux vector, at least in and near equilibrium. From this starting point and using Lorentz invariance he shows that, in equilibrium, s^α is parallel to the flux vector and the stress-energy tensor is $\text{diag}(\mu, p, p, p)$ in the reference frame defined by s^α . Near equilibrium the usual phenomenological laws governing heat conduction, bulk viscosity, and shear viscosity come out as first-order corrections, whereas second-order corrections give the time-derivatives in the diffusion laws which are necessary to limit the diffusion to speeds less than that of light. The treatment of the second-order terms seems to follow that of W. Israel (*Ann. Phys. New York*, **100**, 310–31, 1976) to which Dixon refers, but Israel's "cross-coupling coefficients" don't appear here and it is unfortunate that Dixon does not comment on this. It is also sad that Dixon does not refer earlier in this chapter to the often-overlooked work of van Dantzig in the 1930s, who seems first to have introduced "thermasy" ($T\tau$, where τ is proper time), a variable often used by Dixon as a parameter along the world lines of his fluid.

The final chapter gives, for fluids which interact with electromagnetic fields, a treatment largely parallel to the one above, except that here of course there is no point taking any Newtonian limits. It contains a good discussion of the Abraham and Minkowski stress-energy tensors in view of the arbitrariness of any separation between 'fluid' and 'field' for a polarisable fluid. The second-order terms now include other phenomenological terms associated with the Debye and Langevin effects.

Throughout the book Dixon maintains a clear and methodical style. The book is well-produced and admirably free of misprints, the worst of which I found was a statement (p87) that no timelike vector can be expressed as a linear combination of spacelike vectors, in which it is clear from the context that "orthogonal" is intended before "spacelike". It assumes a fair degree of sophistication of the reader, and I would not send a student to it who did not already know special relativity and relativistic thermodynamics from other sources. But I would recommend it strongly for libraries and for anyone who wants to bring a little more order to that jumbled mixture of theory and phenomenology that most of us carry around in our brains. □

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review article

Dilepton production in hadronic collisions

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Quarks play a central part in particle physics. They were introduced as three fundamental, irreducible building blocks whose combinations reproduced the vast spectroscopy and static properties of hadronic states. Here we are concerned with the extension of the quark concept to collisions: the quark-parton model provides a description of the weak, electromagnetic and strong scattering processes.

A GAUGE invariant quantum field theory of quarks and gluons, quantum chromodynamics (QCD) aspires to the fundamental theory of hadron dynamics. This theory, coopting the successes of the quark-parton model, should account for the quark-quark forces which lead to structure functions (momentum space wave functions) of the bound quarks, including the properties of the 'hydrogen-atoms' of quark-quark forces: the $q\bar{q}$ states of massive quarks. Examples of these systems are the recently discovered particles J/ψ and ψ . We also want this theory to explain 'confinement', why quarks are apparently permanently imprisoned in hadrons.

The field has been enriched by the necessity to postulate an increasing number of quarks. The original three have now been joined by a fourth (charm, 1974) and a fifth (beauty, 1977). I consider here one experimental approach which has been particularly fruitful on two fronts: (1) the enrichment in the number of quarks and (2) on sensitive determinations of the properties of the old quarks which are the main constituents of protons and neutrons.

Dilepton production

There has been much theoretical and experimental concern about the high energy collisions of protons with nuclei in which two leptons appear in the final state:

$$p + N \rightarrow \mu^+ + \mu^- + \text{anything else} \quad (1)$$

(In some experiments the muons are replaced by electrons.) The emerging leptons are the only particles observed in these experiments; their momenta and angles are measured and the 'effective mass' of the pair is calculated:

$$m^2 = 2p^+p^- (1 - \cos[\theta_+ + \theta_-]) \quad (2)$$

As the quantum numbers of the lepton pair are those of a photon, a compilation of the spectrum of effective masses is equivalent to an observation of the (virtual) photon flux generated in high energy collisions.

The effective mass of the muon pair, (that is, the mass of the parent photon) acts as a probe of small distance structure of the colliding hadrons—and the Heisenberg Uncertainty Principle teaches us that the higher the mass, the smaller the detail probed. This was the motivation for the first such experiment carried at the Brookhaven 30 GeV accelerator in 1968¹.

Muons are extremely penetrating, so we can use very intense incident proton beams and protect the detection equipment by thick filters through which only the muons penetrate. Thus one

can select reaction (1) out of processes which are millions of times more probable, and follow the reaction out to large effective masses where the probability of emission is extremely small.

The intuitive idea of probing for small distance structure was quantified by the quark model.

The dynamical property of quarks (the quark picture of high energy collisions) came of age in the Feynman² (partons) and Bjorken³ models in the late 1960s. The first success was the explanation of scaling observed (or so it seemed) in deeply inelastic electron-nucleon scattering. In these models, the hadron is treated as a bound state of elementary, point-like objects, called partons. In the interaction with virtual photons and with the (weak) intermediate bosons, the partons behave as if they are 'almost free'. These ideas are applied to the following reactions:

$$l + N \rightarrow l + \text{anything} \quad (3)$$

$$l + l \rightarrow \text{anything} \quad (4)$$

$$p + N \rightarrow l^+ + l^- + \text{anything} \quad (5)$$

where l can be an electron, muon or a neutrino. 'Anything' is an integration over all possible final states. Reaction (3), already referred to as deeply inelastic scattering has been extensively examined experimentally, beginning with pioneering experiments at SLAC and progressing to much higher energies at Fermilab where the electrons are replaced by muons. In the constituent model, the charged lepton interacts with the quark constituent through the emission and absorption of a photon (virtual) of momentum Q . In this view equation (3) can be written

$$l^- + q \rightarrow l^- + q \quad (6)$$

The experimental result that most significantly advanced the quark model was the confirmation of Bjorken's prediction of scaling. Scaling describes a particularly simple behaviour of the scattering in which only a particular combination of supposedly independent variables are adequate to describe the data: the combination is written as

$$x \equiv \frac{2m_p \nu}{Q^2}$$

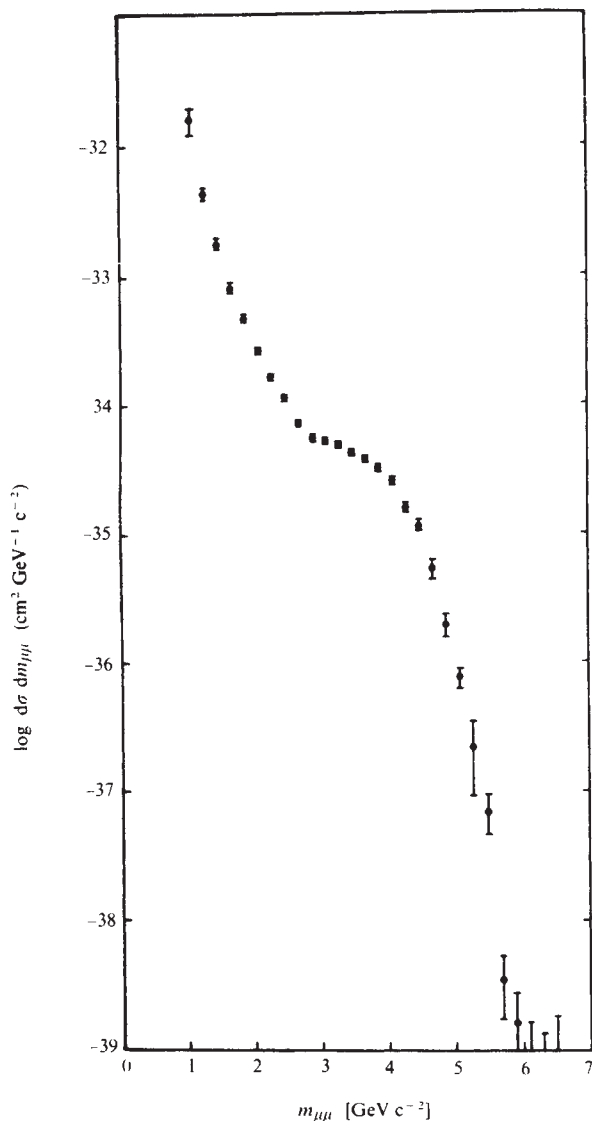


Fig. 1 Data obtained at BNL in 1968 (See ref. 1).

where ν is the lepton's energy loss in the scattering and Q is the momentum transferred, so that the scattering is described by functions:

$$F(x \text{ only}) \quad (7)$$

The scaling behaviour will be shown later to be only approximately true.

Reaction (4) is investigated in electron-positron storage rings. These included facilities at Orsay (France), Frascati (Italy), Novosibirsk (USSR) and, most notably at SLAC's SPEAR and DESY's DORIS. The quark view of equation (4) is:

$$e^+ + e^- \rightarrow q + \bar{q} \quad (8)$$

Given these interpretations, Drell and Yan⁴ interpreted reaction (5) as:

$$q + \bar{q} \rightarrow e^+ + e^- \quad (9)$$

the hard collision of two hadrons to produce a massive lepton pair is viewed as the annihilation of a quark in one with an anti-quark in the other. At first sight it would appear that equations (8) and (9) are simply reversed forms of the same reaction. However, in practice, they teach us quite different things. Quarks do not emerge from collisions but somehow fragment into normal hadrons such as pions, and kaons. Thus reaction (8) informs us about the fragmenting process. Reaction (9) is dominated by the density and motions of the quark and

anti-quark content of the colliding particles and so informs us about the quark structure of hadrons. This would then be the main thrust of dilepton physics except that from time to time, the continuum distribution of muon pairs is interrupted by the resonant production of a massive particle which decays into lepton pairs. This distorted the original Brookhaven spectra¹ but was not perceived because of the poor mass resolution which smeared the mass peak at 3.1 GeV into the shoulder seen in Fig. 1. This peak was found in an improved experiment run in 1974 and named the J/ψ particle⁵. Its interpretation as evidence for the anticipated new quark (the charmed quark) has been discussed in detail elsewhere. In the higher energy experiment carried out at Fermilab in 1975–78, the J/ψ peak is shown in Fig. 2 along with a second interruption to the rational study of the continuum—the upsilon peak near 9.5 GeV. Here again it is generally accepted that a new quark is operating^{6–8}.

Thus, the physics of dilepton production has two components both relevant to the constituent theory of hadrons: (1) the study of the structure functions of the hadrons, dominated by old quarks and (2) the existence, as a very small component, of heavier quarks bearing new internal quantum numbers (flavours).

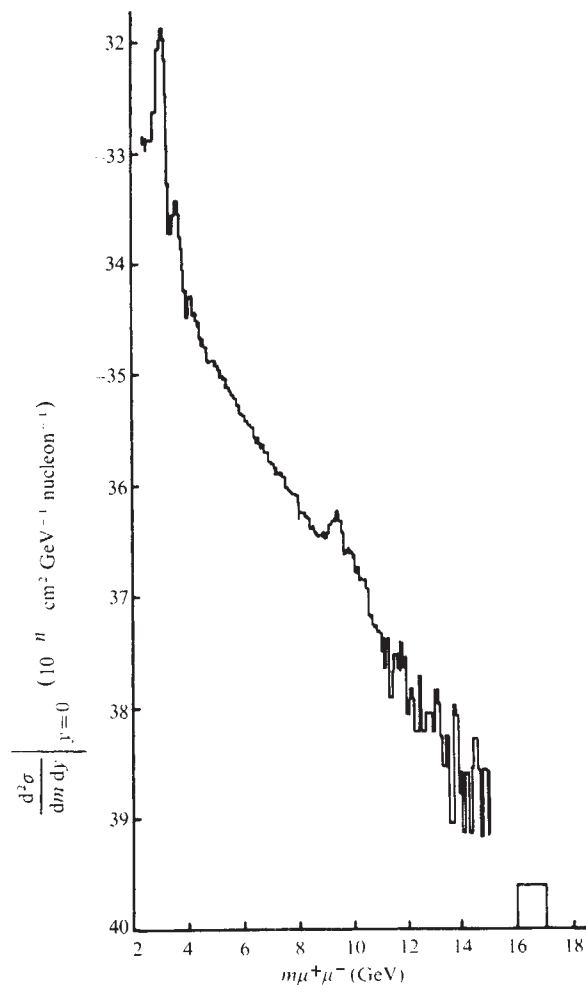


Fig. 2 Dimuon mass spectrum obtained at FNAL in 1977 showing J/ψ family bumps near 3 GeV and the upsilon peaks near 10 GeV. $-0.3 \leq y \leq +0.3$.

Experimental

After several experiments^{9–10} in which electron pairs were detected^{11,12}, the leptons of choice are muons, principally because of the ease of filtering the enormous flux of hadrons. The earliest muon experiments were hampered by poor resolu-

tion—the result of multiple scattering of the muons in the iron that served as a filter. In the most recent Fermilab research^{6–8} iron was replaced by beryllium, a compromise which led to a great increase in sensitivity (combination of intensity and resolution). The experiment then consists of a double arm spectrometer which identifies and measures the vector momenta of the two emerging muons. The effective mass is calculated as in equation (2). Other variables of interest are the longitudinal momentum of the parent virtual photon:

$$P_{11} = P_+ \cos \theta_+ + P_- \cos \theta_- \quad (10)$$

and its transverse momentum:

$$P_1 = P_+ \sin \theta_+ + P_- \sin \theta_- \quad (11)$$

We include here data from three groups of experiments. The Fermilab experiments of a Columbia–Fermilab–Stony Brook (CFS) collaboration used 200, 300 and 400 GeV protons on a copper target, a Fermilab experiment which used 225 GeV pions on a carbon target and a group of experiments at the CERN ISR in which protons colliding with protons gave an effective energy of 2,000 GeV proton–proton collisions.

Resonance production

When the bumps are ignored, the data of Fig. 2, are well fitted with a straight line having a mass distribution of the form

$$\frac{d\Gamma}{d\sigma} = A e^{-bm} \quad (12)$$

Subtracting the fitted curve gives Fig. 3. The fitting program then establishes the existence of two peaks Υ and Υ' at masses of 9.4 and 10.0 respectively. Evidence for a third peak, Υ'' at 10.4 is present and has recently been confirmed. A detailed analysis of the experimental resolution established that the peaks are 'narrow'—the observed width is consistent with the experimental smearing of a peak which must then have a width < 100 MeV and could have essentially zero intrinsic width. This single fact requires a new law of nature. A particle as massive as 10 GeV should decay with extremely short lifetime, corresponding to a width of several GeV unless inhibited by a new law. The standard example of an uninhibited decay, is the rho meson which, at 700 MeV of mass, has a width which is 15% of its mass. A detailed investigation of various candidates found in the theoretical literature ruled out all but one possibility: the upsilon is made of new quarks—and is then strongly inhibited from decaying into pions, kaons and so on—particles made of the old up, down and strange (u, d, s) quarks. This essentially repeated the lesson learned in the case of the J/ψ . There, the new (charm) quark hypothesis was resisted until higher energy e^+e^- collisions produced particles in which the charm quantum number was exhibited. (In the J/ψ , it is hidden by a cancellation—the J/ψ is a $c\bar{c}$ bound state.)

Here we should remember the quantum numbers of the old quarks. They are arranged in pairs:

$$\begin{pmatrix} u \\ d \end{pmatrix} \quad \begin{pmatrix} c \\ s \end{pmatrix} \quad (13)$$

The basic distinction is called the flavour—in these pairings, the upper quark has an electrical charge of $+2/3$, the lower one $-1/3$. A proton is primarily a three-quark system (uud), a neutron is (udd). The upsilon changes the picture to:

$$\begin{pmatrix} u \\ d \end{pmatrix} \quad \begin{pmatrix} c \\ s \end{pmatrix} \quad \begin{pmatrix} ? \\ b \end{pmatrix} \quad (14)$$

where the designation b (bottom or beauty) is assigned to the newest quark. The upsilon is then $b\bar{b}$ bound state in this interpretation. The evidence in favour of this interpretation may be summarised as follows:

(1) We have already noted that the FNAL data on the narrowness of the upsilon peak ($\sigma < 100$ MeV) proves the need for a new quantum number. Weak bosons and Higgs particles were ruled out experimentally^{9,10}.

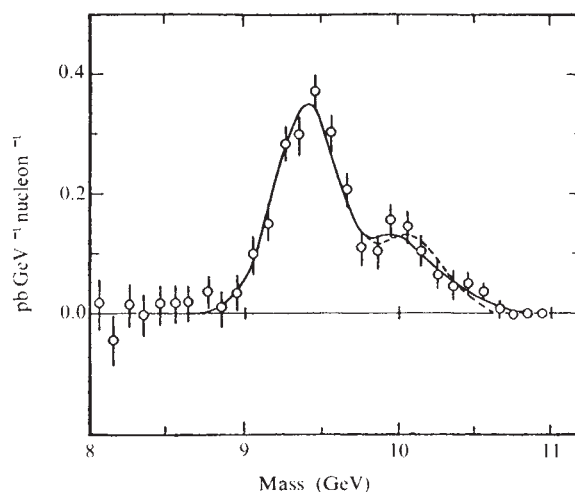


Fig. 3 Upsilon peak with the continuous background subtracted.

(2) The force between quarks, as a basic premise, is supposed to be independent of flavour and carried by quanta of the quark–quark force called gluons. The J/ψ system of particles carrying the c-quark has been analysed in terms of a potential model by Eichten and Gottfried¹³. Using this potential, they calculated that a quark of mass, $m \sim 5$ GeV should give rise to three bound states $1S^3$, $2S^3$ and $3S^3$ of the $b\bar{b}$ system with spacings which are crudely that observed.

(3) Other calculations^{14–18} using the $b\bar{b}$ model successfully accounted for the relative strengths of the three levels.

(4) The above conclusions have recently been confirmed in the reaction (4) at the DORIS storage rings. Here, the partial width of the decay

$$\Upsilon \rightarrow e^+ + e^- \quad (15)$$

is measured and^{19–21}, when compared to theoretical expectations²² establishes the charge $|e_b| = 1/3 e$.

The expectation of partners to the b-quark to replace the ? in equation (14) (called t for top or truth) is very strong. The CFS group has extended the search by collecting ~ 10 times the data that led to the discovery of the upsilon. No more bumps were found and therefore a limit of

$$m_t \geq 8.5 \text{ GeV}$$

could be set by the absence of a bound state, $t\bar{t}$, up to masses of 17 GeV.

Having established the structure of the upsilons as a set of bound states of an 'atom' composed of a massive quark and antiquark, we can learn about the force that produces the binding. This is the primordial strong force, as (see below) our present view of matter is that hadrons are made of quarks glued together by this quark–quark force. (The strong force between protons and neutrons, in this view, becomes a complicated Van der Waal's-type interaction.) Analyses of the upsilon is simplified by the fact that non-relativistic potential theory may be used^{13–18,22}. Literature data indicate that there is a narrow range of potential models that can fit the spectroscopy of both the charmonium and the upsilon energy levels. A very incisive set of data will come from the spectroscopy in the upsilon region and from heavier quark bound states.

The hadrons we know are predominantly composed of u and d quarks. There are traces of s, c, b in decreasing amounts as the masses increase. Nevertheless, without a complete picture of the quark content, an understanding of hadrons is not possible. Clearly an inflation in the number of quarks raises basic questions about elementarity—perhaps six quarks will settle the problem—but already speculations of subquark structures are appearing.

Old quarks: structure functions

The Drell-Yan model (equation (9)), has a simple formulation if we integrate over the motion of the dilepton. As the mass depends only on the momenta of the colliding quarks conservation of energy and momenta permit us to write:

$$x_1 x_2 = \frac{m^2}{s} \equiv \tau \quad (16)$$

$$x_1 - x_2 = x_F \quad (17)$$

$$x_1 = \sqrt{\tau} e^y; x_2 = \sqrt{\tau} e^{-y} \quad (18)$$

Here x_1 is the fraction of the hadron's total momentum carried by the quark 1. Similarly for x_2 . x_F is the longitudinal momentum of the dilepton, m is the mass and s the square of the energy of the collision, evaluated in the CM.

The variable y is often used in place of longitudinal momentum and is known as the rapidity, defined by equation (18).

The aim of the analysis of continuum data is to obtain the structure functions $V(x)$, that is the probability distribution for finding a quark of fractional momentum x . Since there are u and d quarks in a proton, we need $u(x)$, $d(x)$. We note that the proton consists mainly of uud quarks. However, in the old picture of the proton, there was a meson cloud, the virtual presence of mesons, was needed to account for the properties of the proton. In the quark version, the cloud is condensed to a 'sea' of quark-antiquark pairs created by the very strong forces between the main (or valence) quarks. As the sea appears in pairs with no net quantum numbers, we can have $u\bar{u}$, $d\bar{d}$, $s\bar{s}$, $c\bar{c}$, and so on. This accounts for the possibility of producing objects with strange quarks, charmed quarks and so on, in high energy collisions. It also accounts for the presence of antiquarks, required for the Drell-Yan process. Now then, the probability of the production of a dilepton can be written as:

$$\frac{d\sigma}{dm} \propto [V(x_1)S(x_2) + V(x_2)S(x_1)] \quad (19)$$

subject to the condition (16). Here we abbreviate the valence quark distributions by $V(x)$ and the sea quark distributions by $S(x)$.

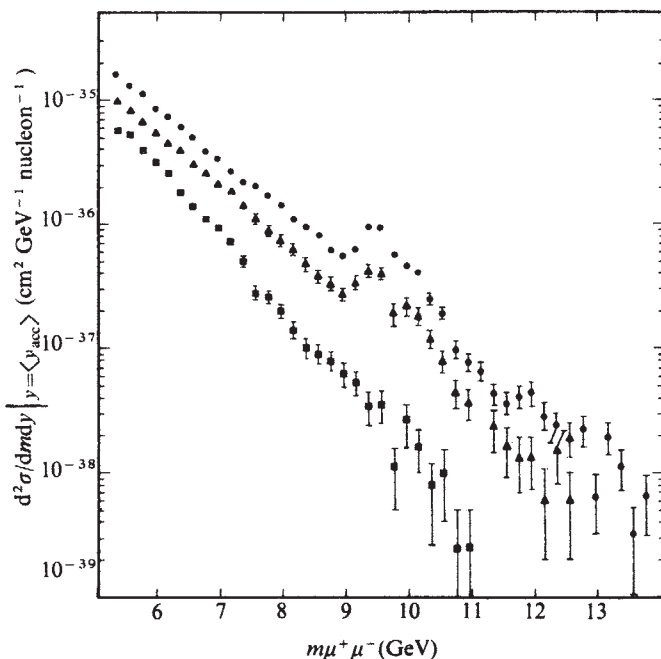


Fig. 4 Dimuon spectra produced by 200 (■), 300 (▲) and 400 (●) GeV proton collisions.

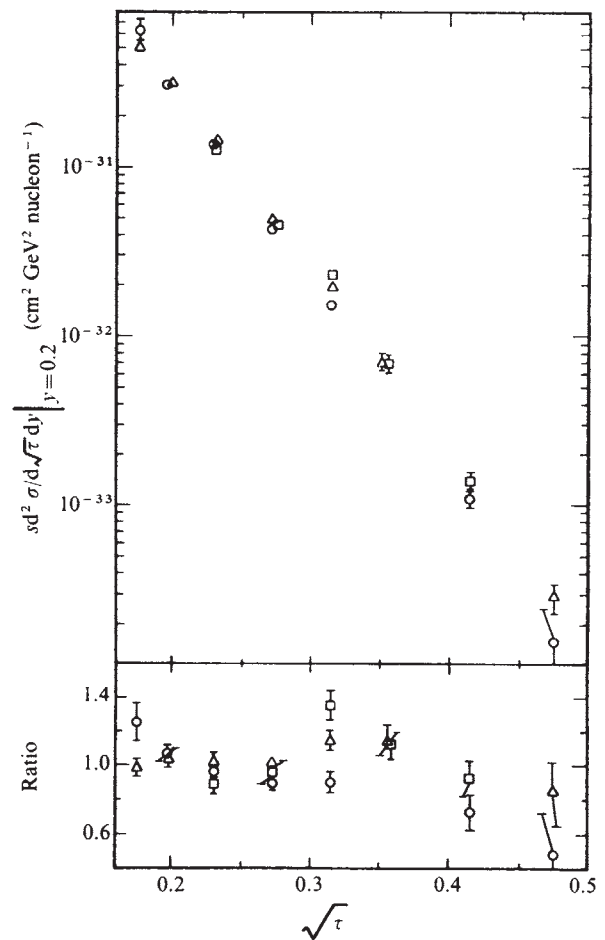


Fig. 5 Test of the scaling hypothesis: at 200 (□), 300 (△) and 400 GeV. All three energies give identical results when $m^2 d\sigma/dm$ is plotted against the dimensionless variable m^2/s .

Now reaction (3), deeply inelastic scattering, also measures $V(x)$ and therefore, the dilepton data of Fig. 2 can be used to determine the sea distribution. A typical result of the fitting program yields a distribution:

$$S(x) = 0.54 (1-x)^{8.5} \quad (20)$$

so that the sea momenta are crowded towards very low values of the fractional momentum. This would be valuable information but the universality of the quark model leads to an even more important conclusion.

There is a reasonable amount of experimental support for the notion that the weak and electromagnetic forces are 'unified'. In older forms, a conserved vector current (CVC) theorem was proposed and tested. The modern version is the theory of Weinberg and of Salam which group the mediators of the weak force, W^+ , W^- , Z^0 and the mediator of the $e-m$ force, γ together into a family. Thus all reactions mediated by a virtual photon should have analogues mediated by the weak boson. Support for this was originally found in weak decays and more recently in neutrino collisions.

The analogue of equation (3) is then

$$\nu + N \rightarrow \mu^- + \text{anything} \quad (21a)$$

or

$$\nu + q \rightarrow \mu^- + q' \quad (21b)$$

for example,

$$\nu + d \rightarrow \mu^- + u$$

Thus neutrino (and anti-neutrino) scattering is also sensitive to quark distributions. Data from Fermilab and CERN have also

been analysed to give the sea distribution. The data agrees well with the results of dilepton production. Here is a subtle detail of the constituent model, the sea distribution of anti-quarks as viewed by two observations (1) neutrinos bringing in virtual W 's to probe the quarks and (2) colliding two hadrons one of which provides a quark to seek out an anti-quark in the other. This is a substantial triumph for the fundamental picture of quark constituents.

The model has another success which has 'practical' applications. One can write the cross-section:

$$\frac{d\sigma}{dm} = \frac{1}{m^3} F(m^2, s) \quad (22)$$

From dimensional arguments F must be dimensionless. Scaling is the assertion that:

$$m^3 \frac{d\sigma}{dm} = F(m^2/s) = F(\tau) \quad (23)$$

that is there are no parameters with energy dimensions which enter. The practical value is that one can use this to predict the yield of masses at future accelerators from results at hand. Example: At FNAL, $s = 750 \text{ GeV}^2$ and the yield of 15 GeV dileptons is known. Thus at a future accelerator, such as ISABELLE, $s = 160,000 \text{ GeV}^2$, one would expect the same value of F for $m = 200 \text{ GeV}$. Because m is the mass of a virtual photon, the predictions can be applied to all states which can connect to photons: pairs of massive leptons, W bosons, quarks, monopoles, and so on.

The test of the scaling hypothesis is indicated in Fig. 4 where a fit to the Fermilab data at 200, 300 and 400 GeV (consistent with scaling but over too small a range to be decisive) is extended to 2,000 GeV and compared to ISR ($s = 3,600 \text{ GeV}^2$) results^{23,24}. To the level of $\sim 20\%$ the agreement is good. The fit that was found is:

$$F(\tau) = 44.4 \exp(-26.6 \tau^{1/2}) \times 10^{-30} \text{ cm}^2$$

Another success of the model becomes evident in the Fermilab experiments of a Chicago-Princeton collaboration²⁵ who use pions as the incident particle. A negative pion is a $\bar{u}d$ combination of quarks. Thus the anti-quark is part of the valence population. Qualitatively this means that dilepton production should be much easier there would be no need to search the sea for antiquarks. Indeed the probability for continuum dilepton production near 10 GeV of mass is more than 200 times bigger for incident pions. As a further benefit, the structure function for the valence quarks of the pion have been determined from the same experiment—so a photograph of the pion is now at hand. As the pion lives only 10^{-8} s and is created in high energy collisions, this is another major achievement. The form of the pion structure function (in momentum space) is:

$$u(x) \approx (1-x)^{1.13} \quad (24)$$

the quark momenta in the pion fall only linearly with x .

Quantum chromodynamics

Given this quantitative success (at the 20% level), it is clear that the next few years will lead to refinements and more incisive tests of the model. At this point we should note that the model has become a theory: quantum chromodynamics (QCD) deals with the quantum field theory of the quark-quark strong force in close analogy to QED. Thus the force between quarks is carried by vector gluons which are also an ingredient of the sea. Being neutral they do not interact directly with photon probes but their ability to scatter with quarks and among themselves leads to corrections to the quark model. Some of these effects have been observed in deeply inelastic scattering as violations of scaling in those experiments. That is, experiments at Fermilab²⁶ using high energy muons have established that reaction (7) is not true but that, in the higher Q^2 domain one must write $F(x, Q^2)$. These same corrections, in the dilepton production data we have

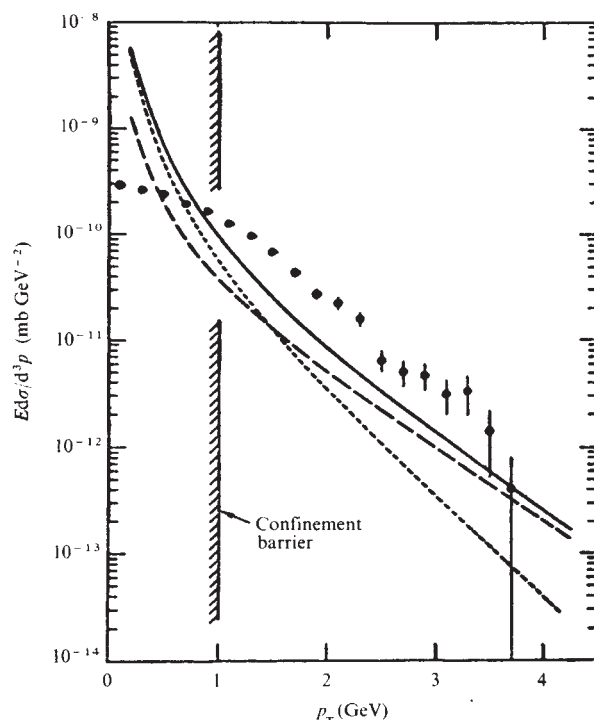


Fig. 6 A comparison of QCD calculations (due to Berger) with the data on transverse momentum of dimuons. We see the infrared difficulties of the present status of the QCD theory, also the theory needs an additional component: intrinsic quark motion: $7 \leq M \leq 8 \text{ GeV}$; $y = 0$; —, total; ---, Compton; - · - · -, annihilation.

discussed are small. However, they show up in a surprising way. There is one aspect of the data which is wholly determined by the QCD effects.

High transverse momentum dihadrons

In the quark-parton model the hadron is viewed as a cluster of parallel moving point-like partons. If two annihilate, the resulting dilepton should also move longitudinally. Even the earliest observations¹ indicated that there is a large transverse momentum component to the dilepton. In the context of the model this implies that the annihilating quarks must bring in the transverse momentum. In the confrontations of theory and data, (see Fig. 6) and in the subsequent evolution of the applications of QCD it became clear that QCD contained just the necessary ingredients: one source of quark transverse momentum has to do with dynamical processes contributing to the production of the lepton pair²⁷⁻³⁵. These processes involving both quarks and gluons could account qualitatively for the observations. Quantitative comparisons with experiment were used to tune the parameters of the theory—most sensitively, the momentum behaviour of the gluons. However, not all the observed effects were produced until an intrinsic transverse momentum was involved. This microcosmic Fermi motion is closely related to the size effect of the confining force.

Thus the transverse momentum of dileptons provides both a basic challenge to the theory and a source of information on the constituent behaviour. Although it is not clear how quantitatively valid the QCD calculations are, we know qualitatively that they give a good account of the high P_T dilepton data, using terms which are required to understand scaling violations observed in reaction (3). The prospects are very encouraging and the issues are crucial. QCD is the first quantum field theory of hadrons. Progress is sporadic and problems in the calculations abound. For example, Fig. 6 shows the infrared divergence of the P_T calculations. Nevertheless, the stakes are high and the advance of the experimental techniques in dilepton physics will provide even more incisive data to confront and guide the developing theory.

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articles

Geochemistry of basalts from the Tyrrhenian Sea

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Comparison between the Tyrrhenian basalts and oceanic basalts coming from different geodynamic environments shows that Tyrrhenian basalts have a clear organic signature with a transitional character.

THE genesis of the western Mediterranean oceanic floor is still poorly understood¹, and the origin of the Tyrrhenian Sea (Fig. 1) is a matter of conjecture. What kind of basement lies underneath—thin continental crust, intermediate crust, or true oceanic crust? Recent studies^{2,3} have interpreted the Tyrrhenian Sea as a marginal one⁴.

The DSPD leg 42 A site 373 A drill hole provided, for the first time, fresh material from the Tyrrhenian Sea floor. This material is basaltic and has recently been described from a petrological point of view^{5,6}. This article presents the results of a geochemical study including strontium and lead isotope compositions and trace elements (including rare earths) concentrations of seven samples representing different stratigraphic positions⁵. These results will permit comparison with the basalts erupted in different geodynamic environments, and are identical to patterns expected for transitional basalts.

Experimental techniques

The strontium isotopic compositions, as well as K, Rb and Sr concentrations, have been determined by mass spectrometry and isotope dilution using the method described elsewhere⁷. The concentrations of these elements are known with an accuracy of 1%. We used the NBS 987 standard and our data are normalised to a $^{87}\text{Sr}/^{86}\text{Sr}$ ratio of 0.7014 ± 0.00007 (2σ) for this standard.

The lead isotopic composition was measured using the Manhes microtechnique⁸. The blanks range from 0.9 to 3.5 ng per 1 g of sample powder. Accuracy of the isotopic measurement is limited by mass fractionation which was found to be 0.1% per mass unit by repetitive analyses of the SRM 982 standard. Data are normalised to the accepted ratios of this standard which are: $^{204}\text{Pb}/^{207}\text{Pb} = 0.05845$, $^{206}\text{Pb}/^{207}\text{Pb} = 2.1433$, $^{208}\text{Pb}/^{207}\text{Pb} =$

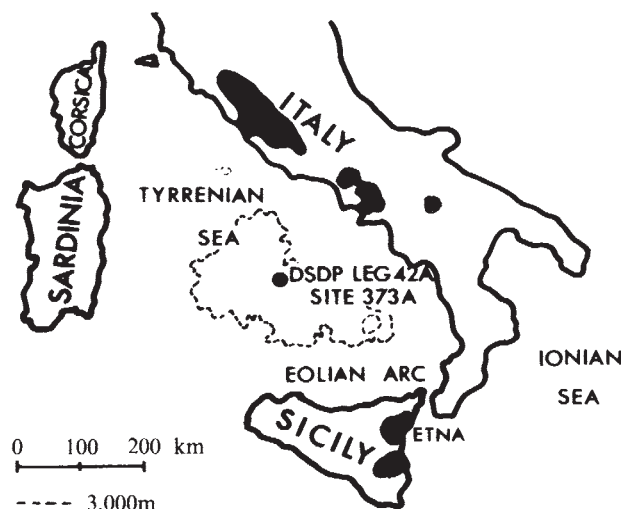


Fig. 1 Location of leg 42 site 373 A. Darkened regions are areas of Italian and Sicilian volcanics.

Table 1 Trace element abundances (p.p.m.) in Tyrrhenian basalts

	1 Core 7-3 130-135	2 Core 2-1 136-142	3 Core 12-1 120-124	4 Core 4-1 87, 5-92	5 Outer Barrel	6 Core 7-2 94-100	7 Core 7-3 100-105
La	4.5 ±0.01	8.0 ±0.1	7.3 ±0.1	5.4 ±0.1	7.5 ±0.2	5.5 ±0.2	4.5 ±0.1
Ce	10.3 ±0.4	18.8 ±0.4	16.5 ±0.3	13.1 ±0.3	18.1 ±0.4	12.8 ±0.3	10.3 ±0.3
Nd	7.3 ±0.5	11.5 ±0.9	12.0 ±1.0	9.2 ±0.9	9.6 ±0.9	8.1 ±0.8	7.2 ±0.8
Sm	2.13 ±0.01	3.60 ±0.02	3.15 ±0.02	2.54 ±0.01	3.5 ±0.02	2.4 ±0.01	2.03 ±0.01
Eu	0.84 ±0.02	1.26 ±0.04	1.22 ±0.03	1.07 ±0.03	1.25 ±0.03	0.93 ±0.04	0.84 ±0.04
Gd	2.6 ±0.2	4.1 ±0.2	4.0 ±0.2	3.0 ±0.2	4.0 ±0.2	2.9 ±0.2	2.5 ±0.2
Tb	0.47 ±0.01	0.75 ±0.01	0.70 ±0.01	0.58 ±0.01	0.74 ±0.01	0.52 ±0.01	0.45 ±0.01
Yb	1.91 ±0.03	2.75 ±0.04	2.75 ±0.04	2.34 ±0.04	2.86 ±0.04	1.94 ±0.03	1.74 ±0.03
Tm	0.35 ±0.03	0.49 ±0.03	0.50 ±0.04	0.40 ±0.04	0.50 ±0.03	0.35 ±0.02	0.31 ±0.02
Lu	0.38 ±0.01	0.54 ±0.01	0.54 ±0.01	0.45 ±0.01	0.56 ±0.01	0.38 ±0.01	0.34 ±0.01
U	0.35	0.23	0.15	0.12	0.20	0.15	0.15
Th	0.49	0.74	0.71	0.43	0.71	0.60	0.46
Zr	50	125	125	84	137	96	67
Hf	1.62	3.07	2.88	2.02	3.00	1.88	1.57
Ta	0.22	0.58	0.54	0.34	0.58	0.29	0.24
Ba	62	65	66	57	57	74	61
Cs	0.13	0.04	0.04	0.41	0.04	0.05	0.17
Sb	0.098	0.036	0.048	0.33	0.038	0.018	0.062
Cr	182	124	125	151	131	213	213
Co	38.6	35.5	33.8	35.5	32.2	40.6	33.0
Ni	86	49	53	47	54	114	83
Sc	28.6	27.7	26.8	33.7	27.8	22.5	24.0
Tb	0.47	0.75	0.70	0.58	0.74	0.52	0.45

Uncertainties, estimated by reproducibility and standard measurements, are about 3% for Sc, Th and Ce, 6% for Cs, Eu, Hf, Ta, Cr, Co, La and Sm, and 10% for the others, except Zr (15%). Quoted errors represent statistical error in counting.

Table 2 K, Rb and Sr contents and strontium and lead isotope ratios in Tyrrhenian basalts

Sample	1 Core 7-3 130-135	2 Core 2-1 136-142	3 Core 12-1 120-124	4 Core 4-1 87, 5-92	5 Outer Barrel	6 Core 7-2 94-100	7 Core 7-3 100-105
K (p.p.m.)		2,700	3,300		2,900	2,500	2,900
Rb (p.p.m.)		2.5	2.96	2.2	3.1	2.8	3.7
Sr (p.p.m.)			175	200	177	214	
K/Rb		1,080	1,114		935	893	783
K/Cs		67,500	82,500		72,500	50,000	17,059
$^{87}\text{Rb}/^{86}\text{Sr}$			0.048	0.031	0.050	0.037	
$^{87}\text{Sr}/^{86}\text{Sr}$	0.70321 ±0.00010	0.70292 ±0.00009	0.70313 ±0.00012	0.70299 ±0.00012	0.70300 ±0.00010	0.70300 ±0.00009	0.70306 ±0.00007
$^{206}\text{Pb}/^{204}\text{Pb}$	18.874 ±0.013	18.657 ±0.020				18.774 ±0.023	18.848 ±0.015
$^{207}\text{Pb}/^{204}\text{Pb}$	15.613 ±0.015	15.567 ±0.021				15.587 ±0.022	15.600 ±0.017
$^{208}\text{Pb}/^{204}\text{Pb}$	38.62 ±0.02	38.47 ±0.08				38.52 ±0.02	38.59 ±0.05

Quoted errors for lead data are $(\sigma^2 + (\delta_m R)^2 \cdot \sigma_e^2)^{1/2}$ where σ is the uncertainty in the run, and σ_e the error on the discrimination factor, estimated from repeated measurements of SRM 981 standard, R is the measured ratio and δ_m the mass difference in this ratio. Measured ratios of one basalt from the Lau basin are: $^{206}\text{Pb}/^{204}\text{Pb} = 18.322$, $^{207}\text{Pb}/^{204}\text{Pb} = 15.521$, $^{208}\text{Pb}/^{204}\text{Pb} = 38.020$.

2.1393. No measured value differs from this average by more than 0.4% for $^{204}\text{Pb}/^{207}\text{Pb}$, which can be considered as an upper limit for error.

The trace element concentrations (other than K, Rb and Sr) have been measured by epithermal neutron activation analysis, following the technique described by Treuil *et al.*⁹. The rare earths were measured after chemical separation, the other elements by direct instrumental analysis. Results and uncertainties for each element are given in Table 1.

Results and interpretations

The $^{87}\text{Sr}/^{86}\text{Sr}$ isotopic ratios are fairly constant (0.7031) (Table 2) and higher than the typical mid-oceanic ridge basalt values, which are between 0.7022 and 0.7028 (refs 10, 11); they are also higher than the few values obtained on interarc basin tholeiites (Lau Basin¹¹ and Mariana Trough^{12,13}). These results are very

similar to the 'abnormal or transitional MORB', like the Reyjanes Ridge near Iceland^{14,15} or the FAMOUS area^{16,17}. On the other hand, the values are much lower than those of the Eolian basalts (0.7049 to 0.7065)¹⁸ and Italian volcanic rocks analysed by Vollmer¹⁹, although similar to the values on the eastern Sicilian volcanics²¹ (Etna and Iblean basalts).

The lead isotopic compositions (Table 2, plotted on a $^{207}\text{Pb}/^{204}\text{Pb}$ versus $^{206}\text{Pb}/^{204}\text{Pb}$ diagram in Fig. 2) fall just above the 'MORB quadrangle' defined by Sun²², as do the FAMOUS area samples (Dupré and C.J.A., in preparation, and Cumming²³). These basalts have $^{206}\text{Pb}/^{204}\text{Pb}$ ratios similar to those of the Icelandic basalts, but slightly higher $^{207}\text{Pb}/^{204}\text{Pb}$ ratios^{15,24}, and are also within the domain of some island arc volcanics^{25,26}. Moreover, they are clearly different from the Italian volcanic rocks¹⁹, as well as from the Sicilian and Etna basalts²¹, which show the high $^{207}\text{Pb}/^{204}\text{Pb}$ typical of rocks

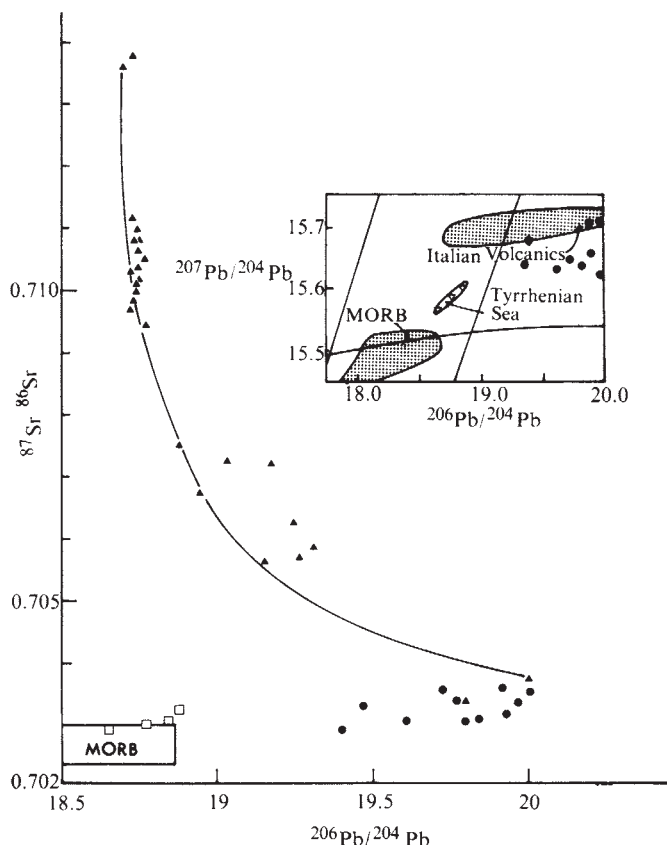


Fig. 2 Lead-strontium correlation diagram discussed by Vollmer¹⁹. The Tyrrhenian basalt results (□) are clearly off the mixing trend between Italian volcanic rocks (▲) and Sicilian and Etna basalts (●).

including crustal material. The few data obtained on marginal basins (Mariana Trough¹³, and one sample from the Lau Basin, Table 2) give typical ridge values.

The Ni and Cr content of basalts which are direct products of partial melting of the mantle are around 220 to 350 p.p.m. for Ni, and 500 p.p.m. for Cr (refs 27, 28). Such values decrease rapidly when the basalts suffer shallow fractionation²⁸. The values of Ni, Cr and Co (Table 1) show clearly that these basalts are not very primitive and have undergone some fractional crystallisation before their eruption on the ocean floor. Therefore, data on trace element levels should be used with caution in attempts to infer the nature of the primitive material. However, findings for the magmatophile elements²⁹, which are the least affected by fractional crystallisation, lead to several interesting points. The K/Rb ratios are similar to those of MORB (~1,000) and clearly higher than values for alkali basalts³⁰: this is true in spite of the high content in Cs (for at least three of the analysed samples), which is a very good alteration indicator. Alteration could have lowered the K/Rb and K/Cs ratios, and raised the K content and ⁸⁷Sr/⁸⁶Sr ratio³¹ (see, for example, sample 7). It is likely that these characteristics have not been greatly affected,

however, as we observe no correlation between the Cs and K contents and K/Rb or Sr isotopic ratio, and as these values are the same for the samples with low Cs content.

The rare earth patterns are very constant, although the absolute concentrations vary. The patterns are light rare earth enriched, very different from typical MORB which show light rare earth depletion. Such a distribution is very similar to the transitional MORB type, like some Icelandic tholeiites or the basalts dredged in the FAMOUS area³². On the other hand, the Tyrrhenian basalt patterns are very similar to those found by Hart *et al.*¹³ in the Mariana Trough.

Th and Ta are incompatible elements and therefore their ratios are not greatly affected by fractional crystallisation and partial melting²⁹. One of the very interesting properties of this ratio is the clear distinction in signature between subduction zone volcanics and oceanic volcanics. The Ta/Th ratio of subduction zone volcanics, like Japan or the Eolian islands, is very low (0.1) whereas the oceanic type like the mid-Atlantic Ridge, Red Sea or FAMOUS area, is between 0.6 and 1.2. The basalts from the Tyrrhenian Sea are clearly of the 'oceanic' type.

The Tyrrhenian Sea is a relatively recent (7.3 Myr)⁵ and rather small sea, bordered by two important cratons, so these results suggest that the development of small or marginal seas may generate oceanic crust containing basalts of transitional character. Such characteristics are different from the marginal sea types associated with subduction zones which are, so far, not very different from typical MORB.

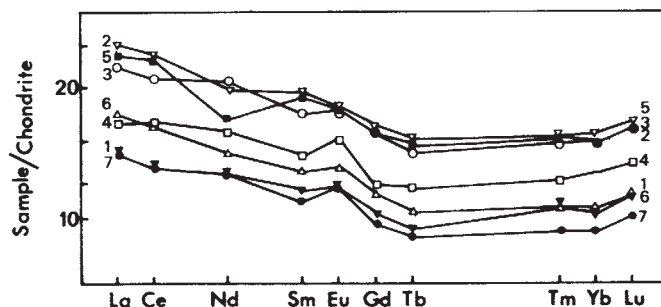


Fig. 3 Chondrite normalised rare earth abundance. The values are similar to those of transitional basalts, like those of the FAMOUS area³². The positive Eu anomaly can be attributed to the presence of plagioclase phenocrysts⁵.

We can try to incorporate our results into the isotopic picture given by Vollmer¹⁹ for the Italian province. As shown in Fig. 2, the Tyrrhenian basalts do not follow the general trend found for the Italian volcanics. We can perhaps define a trend between Etna and Tyrrhenian basalts. Therefore, the petrogenesis of the Italian volcanic province may be explained by the mixing between oceanic island type magma and the continental crust¹⁹ (such an interaction is very similar to a rift valley-type volcanism, for example), although it may not be due to a subduction process. The Etna volcanism may be a result of interaction between the same oceanic island type magma and a mid-oceanic ridge type one.

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Local aggregation of hormone-receptor complexes is required for activation by epidermal growth factor

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An analogue of epidermal growth factor (EGF) which is virtually devoid of biological activity retains receptor binding activity but cannot form cell surface clusters or patches. Bivalent anti-EGF antibodies restore both bioactivity and patch formation. The sensitivity of fibroblasts to native EGF can also be enhanced greatly by these antibodies, especially in hormone-resistant cell lines.

EPIDERMAL GROWTH FACTOR (EGF) is a single-chain polypeptide hormone of molecular weight ~6,000. The hormone was isolated from the male mouse submaxillary gland¹ and found to be a potent mitogen of various epidermal and epithelial tissues both *in vivo* and *in vitro*^{2,3}. It can stimulate the proliferation of various cultured cells such as human fibroblasts⁴, glial cells⁵, granulosa cells⁶ and mouse 3T3 cells⁷. Carpenter and Cohen have presented data which suggest that the binding of EGF to human fibroblasts is followed by internalisation and degradation, presumably by lysosomal enzymes⁸. Using fluorescently labelled, biologically active analogues of EGF and insulin⁹, it has been shown by fluorescent photobleaching recovery that the surface-bound hormone molecules can diffuse laterally in the plane of the membrane with a diffusion coefficient (*D*) of $3-5 \times 10^{-10} \text{ cm}^2 \text{ s}^{-1}$ (refs 9, 10). Fluorescence microscopy studies have shown that these hormones are initially bound diffusely on the cell surface, and at 4 °C they remain dispersed for at least 90 min (ref. 11). Within a few minutes of warming the cells to 37 °C, the hormones aggregate into patches. The visible, clustered hormone molecules are subsequently (within half an hour) internalised by endocytic vesicles. Although it has been tempting to try to relate these processes of redistribution, internalisation and degradation to the mode of action of these hormones, the physiological significance of these events has not been established.

We have recently presented data which indicate that the stimulation of DNA synthesis in fibroblasts by EGF depends on the persistent occupation of a relatively small fraction of the total measurable, externally located binding sites for the hormone¹². The enhancement of DNA synthesis observed 24 h after the addition of EGF is completely blocked by adding anti-EGF immunoglobulin at any time during the first 8–10 h of incubation, indicating the need for the continued presence of externally located molecules¹².

The studies described here suggest that hormone-receptor complexes must undergo some type of cell surface redistribution and self-aggregation to elicit a biological response. Cleavage of EGF with cyanogen bromide results in a unique hormone derivative (CNBr-EGF) which is virtually devoid of biological activity but which nevertheless retains substantial (10%) binding activity. The biological activity of this inactive analogue can be restored by cross-linking with specific bivalent antibodies to EGF. Also, concentrations of native EGF far below those that

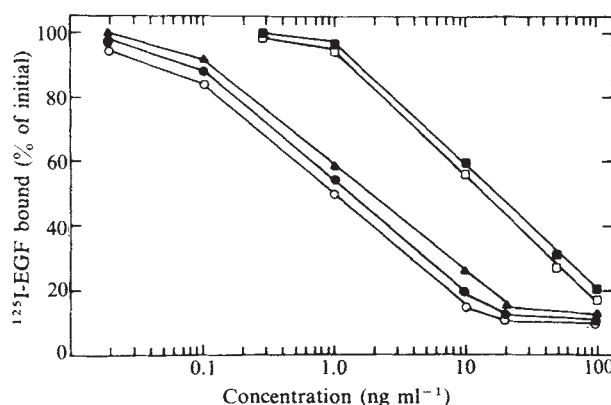


Fig. 1 Competition of binding of labelled native EGF by EGF and various derivatives. 3T3 fibroblasts in monolayers (approximately 3×10^5 cells, 100 μg of protein) and ^{125}I -EGF (1.4 ng ml^{-1} , 300,000 c.p.m. per ng) were incubated with various concentrations of native EGF (purified from mouse submaxillary glands as described in ref. 13) or its derivatives in 0.5 ml medium containing 0.1% bovine serum albumin (BSA) for 45 min at 37 °C. Unbound radioactivity was then removed by washing four times with cold Hank's solution–0.1% BSA. The cells were solubilised with 1.0 ml of 1.0 M NaOH and the radioactive content was determined. The amount of bound radioactivity was 6,000 c.p.m. per well in the absence of unlabelled hormone. ○, Native EGF; ●, native EGF which had been incubated in 70% formic acid for 24 h; ▲, R-EGF; □, CNBr-cleaved EGF; ■, R₂-CNBr-cleaved EGF.

normally show activity can elicit dramatic responses on addition of anti-EGF antibody. Furthermore, cells that are relatively refractory to EGF, despite unimpaired binding, can be markedly 'sensitised' to respond by cross-linking the cell-bound native EGF with antibody. Clearly, hormone binding alone is a biologically unproductive process without a subsequent step of micro-redistribution, aggregation or cross-linking which seems to be a limiting, determining process.

CNBr-EGF: a biologically inactive analogue antagonistic for EGF binding but not activity

EGF has a single methionyl residue at position 21 (ref. 1), and cleavage with cyanogen bromide results in the formation of two polypeptide chains connected by the disulphide bonds of the protein molecule. The binding and biological properties of CNBr-EGF are presented in Fig. 1 and Table 1. Cyanogen bromide-cleaved EGF inhibits the binding of ^{125}I -EGF to 3T3 fibroblasts at concentrations approximately one order of magnitude higher than the native hormone. However, CNBr-EGF only has 0.3% or less of the activity of the native hormone in stimulating thymidine incorporation (DNA synthesis) into

confluent 3T3 fibroblasts. The analogue therefore exhibits at least a 30-fold disparity between its affinity for binding and for biological activation.

The biological and binding properties of the other derivatives used in the present studies are also presented in Table 1 and Fig. 1. Labelling of CNBr-EGF with two molecules of rhodamine (R), to produce R₂-CNBr-EGF, does not result in significant further changes in the properties of this derivative. Exposure of native EGF to the same conditions used in the preparation of CNBr-EGF slightly reduced the binding affinity and biological activity (Table 1).

Cyanogen bromide-cleaved EGF retains 10% of its antigenic activity as determined by radioimmunoassay (see legend to Fig. 2). Half-maximal inhibition of ¹²⁵I-EGF precipitation in this assay is obtained at 50 ng per assay compared with a value of 5 ng per assay obtained with the native hormone.

Anti-EGF antibodies restore the activity of CNBr-EGF

When 3T3 cells are incubated with CNBr-EGF and anti-EGF IgG is then added, full restoration of biological activity can be obtained. Figure 2 shows a typical experiment in which the cells were first incubated with 5 ng ml⁻¹ of the derivative and then with various concentrations of anti-EGF IgG. Full restoration of activity is limited to a narrow range of concentration of the antibody (1:10,000–1:8,000 of the antibody solution, A₂₈₀ = 3.1). No restoration of activity occurs at higher or lower concentrations or when monovalent Fab' derived from anti-EGF IgG is combined with the hormone derivative (Fig. 2). This fragment has about 80% of the capacity of anti-EGF IgG to precipitate ¹²⁵I-EGF in a double antibody immunoassay. Anti-EGF IgG has no effect on DNA synthesis in the absence of the hormone derivative, and an IgG fraction from normal rabbit serum is inactive either alone or in combination with CNBr-EGF.

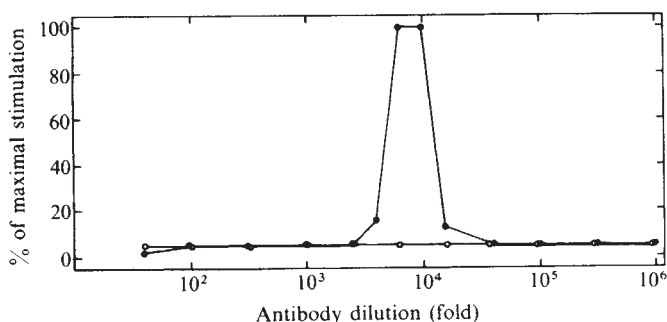


Fig. 2 Biological activity of CNBr-cleaved EGF in the presence of various concentrations of anti-EGF-IgG. Thymidine incorporation into confluent 3T3 monolayers was carried out in the presence of CNBr-cleaved EGF (5 ng ml⁻¹) and the indicated antibody dilutions prepared from purified anti-EGF immunoglobulin fraction (A₂₈₀ = 3.1). The cells were incubated with the hormone derivative for 20 min at room temperature before adding the antibody. The cells in this experiment produced half-maximal stimulation at EGF concentrations of 0.15 ng ml⁻¹. Maximal thymidine incorporation was 400% of control values in the absence of added mitogen. Monovalent Fab' was prepared by papain digestion of anti-EGF-IgG (ref. 15). The Fc fragment and the undigested IgG fraction were removed by a Sepharose-protein A affinity column (Pharmacia) and the non-retarded fraction was subjected to reduction with 5 mM dithiothreitol at pH 8.5 for 30 min. Iodoacetamide was then added (0.01 M final concentration) and after 20 min the sample was dialysed overnight against 0.05 M sodium phosphate (pH 7.5) at 4 °C; CNBr-EGF combined with anti-EGF-IgG (A₂₈₀ = 3.1) (●); CNBr-EGF combined with monovalent Fab' fragment derived from anti-EGF-IgG (○). By radioimmunoassay CNBr-EGF retains 10% activity, as determined by a standard 'double antibody' radioimmunoassay using anti-EGF-IgG fraction (A₂₈₀ = 3.1) as the 'first antibody' at a final dilution of 1:5,000. Anti-EGF-IgG was purified¹² from rabbit anti-EGF serum; this was used rather than anti-EGF serum as the latter has intrinsic DNA-stimulating activity¹².

Table 1 Binding and biological activities of various EGF derivatives

EGF or derivative	Binding affinity to cells		Biological activity	
	50% ¹²⁵ I-hormone displacement (ng ml ⁻¹)	Relative binding potency (%)	ED ₅₀ (ng ml ⁻¹)	Relative activity (%)
EGF	0.8	100	0.13	100
EGF treated with 70% formic acid	1.0	80	0.15	87
R-EGF	1.3	61	0.24	54
CNBr-EGF	10	8	50	0.3
R ₂ -CNBr-EGF	12	7	60	0.2

3T3 fibroblasts were plated at 10⁵ cells per dish in 1 ml of modified Eagle's medium with HY-Clone 10% fetal bovine serum (Sterile System) in 35-mm plastic tissue culture dishes. Incubations were carried out at 37 °C under 90% air, 10% CO₂ humidified atmosphere. The cells were fed after 24 h and used after 96–120 h. Thymidine incorporation into confluent monolayers was measured as described previously¹⁴ using a 2-h pulse with 1 μCi ml⁻¹ of [methyl-³H]thymidine started 24 h after the addition of the mitogen. CNBr-EGF was prepared by treating EGF (1 mg ml⁻¹) containing a trace quantity of ¹²⁵I-EGF in 70% formic acid with 400–500 equivalents of crystalline cyanogen bromide (Fluka) for 24 h at room temperature. The solution was then evaporated under vacuum and the residue redissolved in H₂O and lyophilised. Native EGF, with no cyanogen bromide added, was treated similarly. An acid hydrolysate of the derivative contained no methionine and about 1 mol of homoserine or its lactone. Details of the preparation of the fluorescent derivatives is given in the legend to Fig. 3. Experimental values for binding affinity were obtained from Fig. 1. The relative binding potency is the concentration of native EGF or derivative that inhibits 50% of the binding of ¹²⁵I-EGF. The ED₅₀ is the concentration of EGF that produces half-maximal stimulation of DNA synthesis. R-EGF = native EGF labelled with one molecule of rhodamine; R₂-CNBr-EGF = CNBr-EGF labelled with two molecules of rhodamine.

These data suggest that the hormone receptors occupied by CNBr-EGF are ineffective because of their inability to induce receptor redistribution and aggregation, and that this deficiency can be corrected by passively cross-linking the CNBr-EGF-occupied receptors with bivalent (but not monovalent) antibody. This hypothesis was explored further by fluorescence microscopy studies.

Distribution of fluorescent analogues of EGF on the cell surface

It was previously reported that in 3T3 cells exposed to rhodamine-labelled EGF (R-EGF) the hormone-receptor complexes are homogeneously distributed at 4 °C whereas at 37 °C they rapidly cluster and form visual patches on the cell surface^{11,16}. When 3T3 cells are incubated with 200 ng per ml of CNBr-EGF labelled with two molecules of rhodamine (at each of its two amino-terminal groups) (R₂-CNBr-EGF) for 45 min at 4 °C, the hormone complexes are also diffusely distributed on the cell membrane (not shown). In contrast to the results with R-EGF, incubation at 37 °C for 1 h or more does not cause any visible clustering, and staining remains diffuse (Fig. 3a, b). However, visual patches are readily seen when anti-EGF antibodies are added to cells labelled with R₂-CNBr-EGF (Fig. 3c, d). These visual patches are observed with antibody dilutions of 1:10–1:100 but not at higher (1:120–1:20,000) dilutions.

These data further confirm that, unlike the native hormone, CNBr-EGF has lost its ability to self-aggregate spontaneously at 37 °C, even at high receptor occupancy and after prolonged periods of incubation. This deficiency can be at least partially corrected by antibody to EGF. Visible patches are not, however, observed at the range of antibody concentration in which the biological activity of CNBr-EGF is restored (see previous section). It is thus possible that microaggregates (which may be composed of two or several hormone-receptor complexes), rather than large clusters or aggregates, are the 'state of microaggregation' responsible for triggering biological activity. It is not possible, however, to determine whether some macroaggregates of the type visualised at high receptor occupancy with R-EGF can also form at low receptor occupancies.

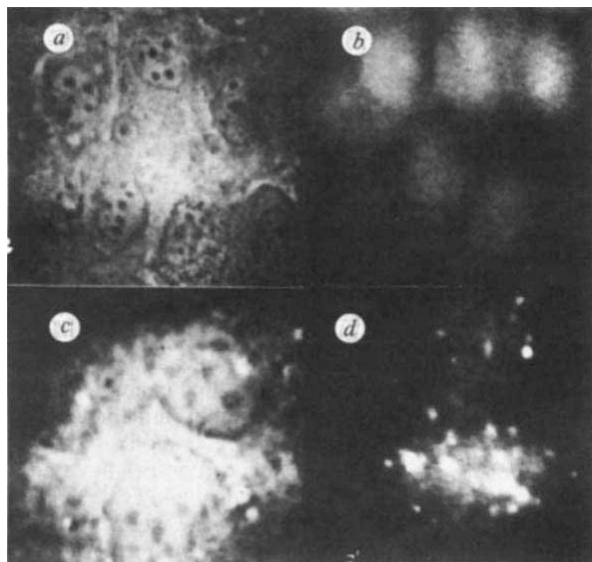


Fig. 3 Distribution of R_2 -CNBr-EGF on the cell surface of 3T3 fibroblasts in the presence and absence of anti-EGF-IgG. *a* And *b* describe the phase and fluorescence micrographs, respectively, of the same field ($\times 1,950$) of 3T3 cells which were incubated for 30 min at 37°C with 200 ng per ml of R_2 -CNBr-EGF. The cells were then washed with phosphate-buffered saline (PBS)-1% BSA, fixed with 1% formaldehyde, and observed with an image-intensified video fluorescent microscope as described previously¹¹. The level of nonspecific fluorescence was determined as described¹¹, and found to be about 30% of the total fluorescence intensity. R_2 -CNBr-EGF was prepared by reacting CNBr-EGF (1 mg) containing a trace quantity of ^{125}I -CNBr-EGF in 1 ml of 0.2 M sodium carbonate buffer (pH 9.3) with three aliquots (20 μl each) of a solution (10 mg ml^{-1}) of rhodamine isothiocyanate (Research Organic Chemicals) in dimethylformamide. Excess reagent was removed by gel filtration on a Sephadex G-15 column. R-EGF was prepared in a similar manner, starting from native EGF (1 mg) containing a trace quantity of ^{125}I -EGF. The ratio of $^{125}\text{I}/A_{545}$ was used to determine the extent of the reaction as previously described⁹. R-EGF and R_2 -CNBr-EGF contained about 1 and 2 mol of rhodamine per mol EGF, respectively. Their biological properties are given in Table 1. *c* And *d* describe the phase and the fluorescence micrographs, respectively, of the same field ($\times 1,950$) of cells prepared as described above except that the cells were incubated for an additional 30 min at 37°C with anti-EGF-IgG (final dilution of 1:50 from a stock solution of the antibody, $A_{280} = 3.1$) before fixation with formaldehyde.

Antibody enhancement of native EGF activity in sensitive and resistant cells

In view of the above findings, we examined the possibility that antibodies might enhance the activity of very low, sub-optimal concentrations of native EGF. In these conditions, it is theoretically possible that the density of active EGF-receptor complexes is sufficiently low to limit the probability and extent of cross-linking in a way that may be enhanced by bivalent antibody. Indeed, although antibodies have virtually no enhancing effects on low but slightly active concentrations (1 pg ml^{-1}) of native EGF, a definite effect on inducing the emergence of activity can be observed when lower concentrations (0.1 pg ml^{-1}) of the hormone are used (Fig. 4a).

As these studies were carried out on cells which are highly sensitive to EGF and whose receptors are not expected to be defective in the property of self-aggregation when occupied by the hormone, we examined cells containing unaltered numbers of EGF binding sites but which were relatively insensitive to the hormone (Table 2). It was reasoned that the receptors in these cells could theoretically be relatively defective in the property of

self-aggregation, so that artificial, exogenous cross-linking with antibody might induce even more dramatic effects. As we have consistently observed that 3T3 cells of low passage are most sensitive to the effects of EGF, and that with increasing passage there is a progressive loss of sensitivity, despite the fact that the numbers of EGF binding sites do not change, high-passage or 'poorly responsive' 3T3 cells were studied (Table 2, Fig. 4b). In these cells addition of anti-EGF IgG results in a dramatic restoration of the activity at all concentrations of the hormone. Highly significant biological effects are observed at EGF concentrations as low as 0.1 pg ml^{-1} , which is three to four orders of magnitude lower than the concentrations normally required to produce half-maximal effects in these cells. As observed for CNBr-EGF, monovalent Fab' is without effect (Fig. 5b). Note that quantitative differences exist between the restoration of activity of CNBr-EGF (Fig. 2) and that observed with submaximal doses of the native hormone (Fig. 4). The restoration is more complete for CNBr-EGF and it is restricted, for unknown reasons, to a narrower range of concentration of antibody. It is also pertinent that substantially lower concentrations of EGF are required to elicit activity in sensitive compared with resistant cells (Table 2). These results also suggest that the loss of hormone responsiveness observed with increased cell passage may be related to the modification of a process which restricts hormone-receptor mobility or self-aggregation.

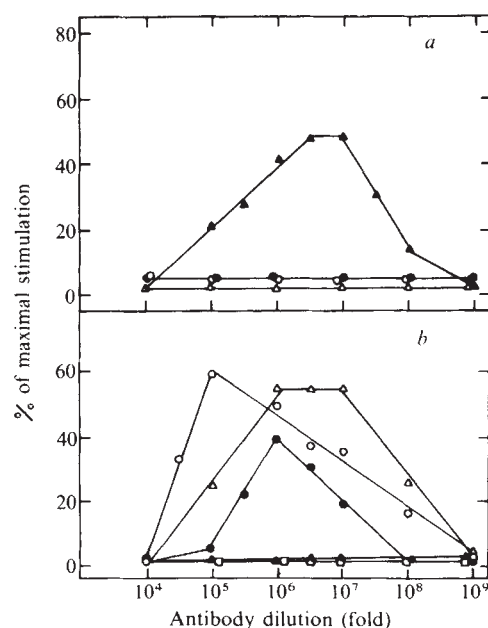


Fig. 4 Biological activity of submaximal concentrations of native EGF using various concentrations of anti-EGF-IgG in 'highly responsive' (*a*) and 'poorly responsive' (*b*) 3T3 cells. *a*, Thymidine incorporation was carried out with 0.1 pg per ml (Δ) and 1 pg per ml ($\bullet$) of EGF and the indicated dilutions of a solution of purified anti-EGF-IgG ($A_{280} = 3.1$). The cells were incubated with the hormone for 20 min before the addition of antibody. The cells in this experiment (passage 2) produced half-maximal stimulation at EGF concentrations of $7\text{--}10 \text{ pg ml}^{-1}$. Maximal thymidine incorporation was 650% of control values without added mitogen. 0.1 pg per ml (Δ) and 1 pg per ml ($\bullet$) of EGF in the absence of anti-EGF-IgG. No effect of the antibody was detected at higher concentrations of the hormone (5 and 10 pg ml^{-1} , not shown). *b*, Thymidine incorporation was carried out with 0.1 pg per ml (Δ), 1 pg per ml ($\bullet$) and 10 pg per ml ($\circ$) of native EGF and the indicated dilutions of purified anti-EGF-IgG ($A_{280} = 3.1$). The cells in this experiment (passage 14) produced half-maximal stimulation at EGF concentrations of 1 ng ml^{-1} . Maximal thymidine incorporation was 250% of control values without added mitogen. $\square$, Native EGF (10 pg) in the absence of anti-EGF-IgG; Δ , EGF (10 pg) plus monovalent Fab'.

Table 2 Comparison of some properties of 3T3 cells 'poorly responsive' or 'highly responsive' to EGF

Cell type	Passage no	No. of binding sites	Binding affinity	ED ₅₀	% Thymidine incorporation
Highly responsive	2-5	40,000-70,000	2.5-4.0	5-20	600-1,100
Poorly responsive	12-26	40,000-70,000	2.5-4.0	1,000-2,000	170-250

Binding affinity is the concentration of ^{125}I -EGF ($\times 10^{10}$ M) required to achieve half-maximal binding, determined by incubating 3T3 cells in monolayer with increasing concentrations of ^{125}I -EGF (300,000 c.p.m. per ng) for 45 min at 37 °C, as described in ref. 14. The ED₅₀ is the concentration (pg ml⁻¹) of EGF producing half-maximal stimulation of DNA synthesis. Thymidine incorporation is expressed as the per cent stimulation with maximal concentrations of EGF compared with that in the absence of the mitogen (100%).

Mechanisms which involve self-aggregation require lateral mobility of receptor sites in the plane of the plasma membrane. This was predicted earlier^{17,18} and demonstrated recently for EGF and insulin receptors of 3T3 fibroblasts^{9,10}. The significance of receptor-receptor aggregation has previously been suggested in receptor-mediated processes as well as in other biological systems. Kahn *et al.* have shown that antibodies to insulin receptors (obtained from patients with insulin-resis-

tant diabetes) can mimic insulin in stimulating glucose oxidation in adipocytes^{19,20}. The Fab' fragment derived from these antibodies competes with ^{125}I -insulin for binding but does not stimulate glucose oxidation. However, cross-linking with anti-Fab' antisera restores activity²⁰. Antibodies raised against the purified insulin receptor²¹, and plant lectins which are known to bind to insulin receptors²² may also produce biological responses by cross-linking insulin receptors. Conditions which cause degranulation of mast cells and basophils also cause clustering of Fc receptors²³. When B lymphocytes, which contain immunoglobulin molecules as an integral component of the plasma membrane, interact with bivalent antigens, they are triggered to proliferate and differentiate²⁴.

The present studies could have important applications to other polypeptide hormone systems. For example, in certain types of insulin-resistant diabetes, or other similar states, defects may exist that impair receptor self-aggregation.

Note added in proof: Anti-insulin antibodies have now also been shown to enhance substantially the biological effects of this hormone on fibroblasts²⁵. Although this effect is accompanied by an increase in the apparent affinity of binding of insulin, in the case of EGF, antibodies do not significantly increase the binding of this hormone to fibroblasts or liver membranes²⁵.

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letters

Circular polarisation and the magnetic dipole model

THE magnetic dipole model for a compact extragalactic radio source proposed by Sanders^{1,2} and discussed by Milgrom and Bahcall³ and by Sanders and DaCosta⁴, accounts for one of the features observed in a few of these sources—the apparently superluminal separation of two components (see ref. 5). However, the magnetic dipole model requires a strong, well-ordered magnetic field and predicts that the radio emission will have a substantial degree of circular polarisation unless the double source appears highly symmetric to the observer, or the radiation is by positrons and electrons in nearly equal numbers. For the past five years we have been monitoring extragalactic variable radio sources for circular polarisation at 8 GHz, and we have recently begun observations at 4.8 GHz. We present evidence here that the circular polarisation observations made by us and by other observers conflict with the predictions of the magnetic dipole model.

Our circular polarisation monitoring program and the observing procedure has already been described⁶. Averages for all our observations of several sources at 8 GHz between

October 1973 and October 1978 are presented in Table 1. The standard deviations are based on the scatter of the observations about their mean values and therefore reflect any possible

Table 1 8.0 GHz circular polarisation measurements October 1973 to October 1978

Source	V (%)	σ_v (%)	N†
0235+164	0.040	0.096	30
3C84	-0.075	0.015	109
3C120	0.031	0.030	64
0J287	0.056	0.090	42
4C39.25	0.031	0.024	80
3C273	-0.056	0.010	110
3C279	0.148	0.017	63
3C345	-0.058	0.021	127
2134+004	0.065	0.031	39
BL Lac	0.168	0.078	20
3C454.3	0.036	0.022	68

* Significant variations in V were detected during the observing period⁶.

† Number of days observed.

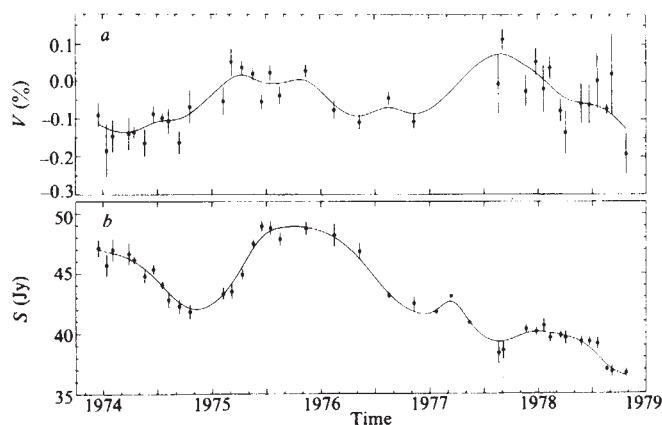


Fig. 1 Monthly averages of the degree of circular polarisation (a) and total flux density (b) for 3C273 at 8 GHz.

variations. Four bright radio sources have exhibited superluminal expansion⁵: 3C120, 3C273, 3C279 and 3C345. The degrees of circular polarisation which we have observed at 8 GHz for these and other sources have been less than or about 0.1% (ref. 6 and Table 1). Degrees of circular polarisation up to a few tenths of a per cent have been detected by other observers at lower frequencies (see refs 7–9). We have detected variations in the circular polarisation from at least two sources, 3C84 and 3C273 (ref. 6). Figures 1–3 show monthly averages of the total flux densities and degrees of circular polarisation at 8 GHz for 3C273, 3C279 and 3C345 respectively. Smoothing cubic splines¹⁰ have been fitted to the data. Throughout the period of observation the degree of circular polarisation did not rise substantially above ~0.1%.

In the magnetic dipole model^{1–4} the relativistic electrons stream out along the magnetic field lines with moderately small pitch angles during an outburst. The required magnetic field strengths must be at least the equipartition value to maintain the geometry of the field. The four sources which have shown superluminal expansion have estimated equipartition magnetic field strengths of about 0.1–1 G (ref. 11). Fields of this strength and pitch angles of 20°–30° yield 1–3% circular polarisation at 8 GHz for a transparent synchrotron source¹². (Sanders and DaCosta⁴ suggest larger magnetic fields and smaller pitch angles, which would increase the degree of circular polarisation to 10–15%.) In contrast, very long baseline interferometry (VLBI) indicates typical values of $\leq 10^{-3}$ G for the transverse component of the magnetic field $B_{\perp}$ in these sources¹³. This can be reconciled with the equipartition values if the pitch angles are quite small, $\leq 10^{-3}$ rad, which would produce degrees of circular polarisation approaching 100%¹⁴.

As Sanders and DaCosta⁴ pointed out, no circular polarisa-

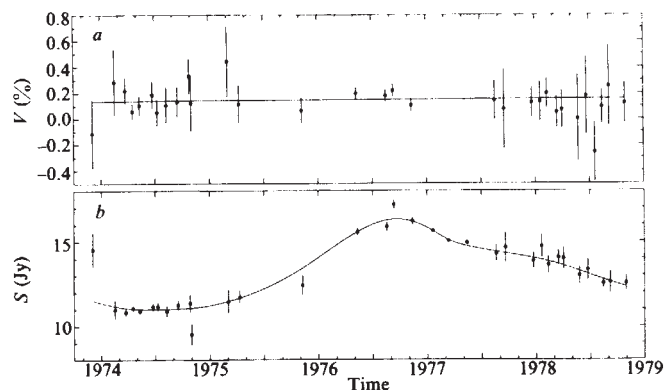


Fig. 2 As Fig. 1 but for 3C279.

tion would be produced if the radiation were generated by comparable ensembles of electrons and positrons. The following discussion considers the case when the radiation is primarily by electrons, so that the individual source components would be highly circularly polarised.

A significant degree of cancellation of the circular polarisation is expected in the magnetic dipole model because the magnetic field points towards the observer in one component and away from the observer in the other component, so that the sense of circular polarisation will be opposite in the two components^{1,4,12}. The net circular polarisation will, in general, still be non-zero because the total flux densities and magnetic field strengths of the two emitting regions are not expected to be identical. For an observer out of the equatorial plane of the dipole (as is required for separation rates in excess of $4.4c$), the magnetic fields and pitch angles in the two components would be different even if the dipole geometry were perfectly symmetrical³. The polarised flux densities of the two components must differ by less than about 10% to be consistent with our observations, but the possibility that the known superluminally expanding sources are highly symmetric is excluded by VLBI^{15–18}. For example, Shaffer *et al.*¹⁵ found that the two

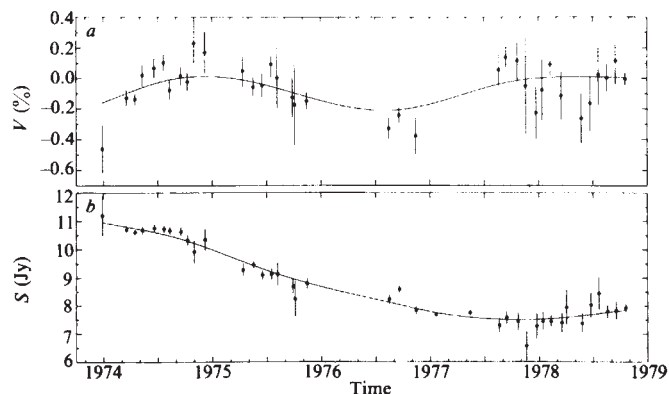


Fig. 3 As Fig. 1 but for 3C345.

components of 3C345 differed by more than 10% in total flux density and that one component had $B_{\perp} \sim 1$ mG where the other had $B_{\perp} \sim 100$ mG.

The conflict between the model and the observations can be reduced if both source components are opaque to the observer. The degree of circular polarisation from an opaque synchrotron source is about one-third as much as from a transparent source¹⁹. However, unless the observer is in the equatorial plane of the dipole, the two components will become transparent at different times (because of the different distances of the emitting regions from the dipole centre), thus producing a large imbalance between the components some time during the development of the burst. Our observations (see Figs 1–3) show no evidence for periods of large circular polarisation. Except for the possibility that the radiation from extragalactic sources is generated by both positrons and electrons, the asymmetries observed by VLBI together with our measurements of low degrees of circular polarisation in these sources do not seem to be reconcilable with the magnetic dipole model.

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Tropospheric circulation and interplanetary magnetic sector boundaries followed by MeV proton streams

TRANSITS of some interplanetary magnetic sector boundaries across the Earth were shown by Svestka *et al.*¹ to be followed by streams of protons with energies in the range of a few MeV. These proton boundary transits were associated with a characteristic decrease²⁻⁴ of the vorticity area index (VAI) during Northern Hemisphere winter that was about twice as large as the decrease associated with all the other boundary transits during the same interval. I show here that the MeV proton streams associated by Svestka *et al.*¹ with certain sector boundary transits apparently select boundary transits associated with unusually active conditions in the solar wind. These proton boundary transits were associated with a larger change in tropospheric circulation than were the other boundary transits observed during the same interval of time. New information in the present study of sector boundary transits relating changes in solar wind velocity, density and magnetic field to the resulting terrestrial changes in vorticity area index, geomagnetic activity and cosmic ray flux at Earth may provide a test for proposed physical mechanisms.

Svestka *et al.*¹ summarised what they observed: "There are certain magnetic sectors in interplanetary space in which the density of low-energy protons is increased. The increased proton flux is first observed when the western boundary of the sector passes across the Earth. After that time the flux remains enhanced for two or more days. In some cases the duration of the enhancement corresponds to the width of the magnetic sector, but generally this is not the rule; the duration is usually shorter, but it can be even longer than the sector width. In some cases the enhancement is observed once again when the sector returns to Earth after 27 days. More often, however, the proton increase is not recurrent."

Table 1 of Svestka *et al.*¹ gives a list of the transits of magnetic sector boundaries associated with MEV proton streams. The overlap in time between this table and the interval studied in refs 2-4 includes the winters in 1963-69 during which there were 18 proton boundary transits. During the same winter months there were 62 other well defined boundary transits tabulated by Svalgaard^{5,6}.

Figure 1a shows a superposed epoch analysis of the classical VAI of Roberts and Olson⁷ computed at 500 mb in the Northern Hemisphere for the 18 proton boundary transits described above (solid line) and for the 62 other boundary transits in the same interval of time (dashed line). The proton boundary transits were associated with a narrower minimum that was about twice as deep as the minimum associated with the other boundary transits.

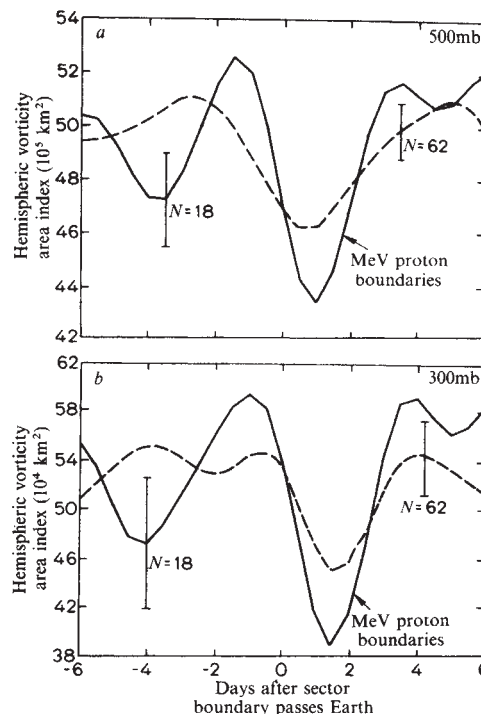


Fig. 1 *a*, Superposed epoch of the vorticity area index during Northern Hemisphere winters 1963-69 about sector boundary transits. The solid curve represents MeV proton boundary transits (see text) and the dashed line represents all other boundary transits in the same time interval. The VAI was computed at 500 mb as the sum of the area in which vorticity was greater than or equal to $20 \times 10^{-5} \text{ s}^{-1}$ plus the area in which vorticity was $\geq 24 \times 10^{-5} \text{ s}^{-1}$. In all figures the error bars indicate $\pm \text{s.e.m.}$ *b*, The same as (*a*) except that the vorticity area index was computed at 300 mb for the area in which vorticity was $\geq 28 \times 10^{-5} \text{ s}^{-1}$. Note that the ordinate in (*b*) is about 10 times smaller than that in (*a*).

As the depth of the minimum in the VAI may depend on the number of boundary transits considered, an analysis was made using 1,000 groups of 18 boundary transit times selected at random from the 62 'other' transits. A quantitative measure of the depth D of the minimum is defined as the average VAI at day -2 and at day 4 minus the VAI at day 1. (This definition is based on the minimum observed from 162 transits during the winters from 1 November 1963 to 31 March 1976.) For each of the 1,000 groups of 18 transit times the value of D is computed. For the 1,000 values of D the mean is $3.54 (\times 10^5 \text{ km}^2)$ and the standard deviation is 2.04. For the solid curve in Fig. 1a the value of D is 7.32, which is almost two standard deviations larger than the mean. This is consistent with the fact that only 30 (3%) of the 1,000 random values of D were larger than 7.32.

Figure 1b is the same as Fig. 1a except that the VAI was computed in a different part of the troposphere, namely at a height of 300 mb for vorticities $\geq 28 \times 10^{-5} \text{ s}^{-1}$. The VAI in Fig. 1a was computed at 500 mb for the area with vorticity $\geq 20 \times 10^{-5} \text{ s}^{-1}$ plus the area with vorticity $\geq 24 \times 10^{-5} \text{ s}^{-1}$. Note that the area of the troposphere computed in Fig. 1b is about 10 times smaller than in Fig. 1a, but in Fig. 1b the minimum associated with MeV proton boundary transits is again at least twice as deep as the minimum associated with the other boundaries. A very similar effect was thus observed in rather different conditions in the troposphere.

Figure 2 shows a superposed epoch analysis of geomagnetic activity (Mudyaud's index⁸ am) associated with the proton boundary transits (solid line) and the other boundary transits (dashed line). There was a large increase of geomagnetic activity following the proton boundary transits and no significant change

following the other boundary transits. Figure 3 shows a superposed epoch analysis of the magnitude of the observed interplanetary magnetic field associated with proton boundary transits (solid line) and the other boundary transits (dashed line). The field magnitude showed a large increase after the transit of the proton boundaries and no significant change associated with the transit of the other boundaries. The shapes of Figs 2 and 3 are very similar, as would be expected from earlier discussions of the response of geomagnetic activity on this time scale to the solar wind and interplanetary magnetic field⁹. A measure of the information content in the present analysis can be gained from the close similarity of the peaks in the solid lines of Figs 2 and 3.

The MeV proton streams discussed by Svestka *et al.*¹ are now thought to have been accelerated in the solar wind beyond 1 AU in co-rotating interaction regions¹⁰ that are formed when high-velocity solar wind streams are catching up with lower velocity solar wind. The MeV proton boundary transits were thus associated with unusually active solar wind conditions.

Some information about possible physical mechanisms may be inferred from the analysis reported here. The magnitude of the interplanetary magnetic field had a large increase after proton boundary transits and no significant change after the

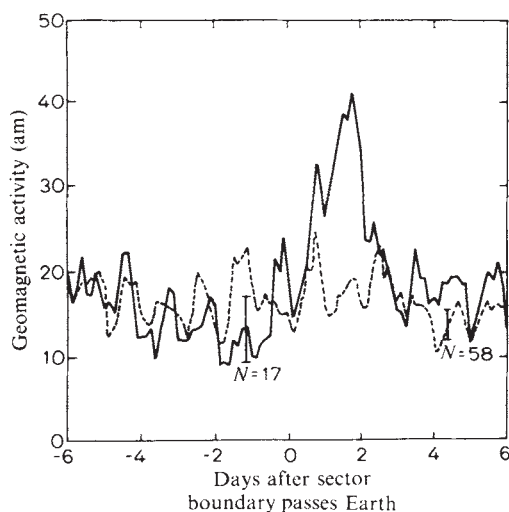


Fig. 2 Superposed epoch analysis of geomagnetic activity associated with the transit of proton sector boundaries (solid line) and the other boundaries in the same time interval (dashed line).

other boundary transits, while the minimum in VAI after proton boundary transits was twice as deep as the minimum after the other boundary transits. But the minimum in VAI after the other boundary transits was still well-defined. Thus, a simple relationship between the increase in the magnitude of the interplanetary magnetic field and the decrease in VAI following boundary transits seems to be excluded.

Geomagnetic activity had a large increase following proton boundary transits and no significant change associated with the other boundary transits, while the minimum in VAI following proton boundary transits and following the other boundary transits was well defined. We may infer that an increased in geomagnetic activity following a boundary transit is not a necessary condition for a minimum in VAI following a boundary transit. Olsen *et al.*¹¹ have reported a relationship between minimum VAI and maximum geomagnetic activity, but the large increases in geomagnetic activity studied by Olson *et al.*¹¹ were associated with transient solar flares (and/or plagues¹²) while the smaller increases in geomagnetic activity discussed in the present paper were associated with the quasi-stationary

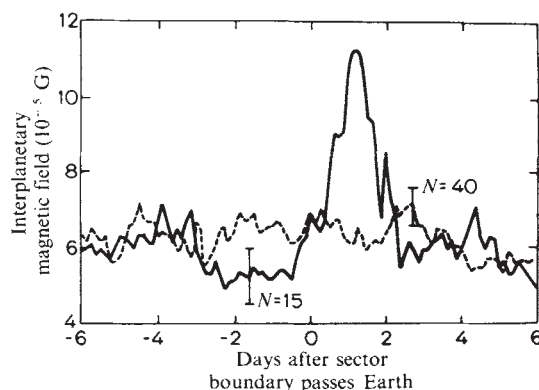


Fig. 3 Superposed epoch analysis of the magnitude of the interplanetary magnetic field associated with the transit of proton boundaries (solid line) and all other boundaries in the same interval (dashed line). Note the similarity between Figs 2 and 3.

co-rotating solar and interplanetary magnetic sector structure¹³. Physical processes may be different in these two cases.

The streams of MeV protons used by Svestka *et al.*¹ to select the proton boundaries analysed in the present work are not necessarily involved in the physical cause of the decrease of VAI following boundary transits. The proton streams may simply mark the presence of unusually disturbed solar wind conditions. Superposed epoch analysis (not shown) of solar wind velocity and of the density showed the characteristic increase¹³ in these quantities following proton boundary transits and following the other boundary transits, but the maximum values were larger after the proton boundary transits than after the other boundary transits. Thus, the increase in solar wind velocity and/or density that followed boundary transits may have been associated with the minima in VAI that followed boundary transits. The galactic cosmic ray intensity observed at Earth (Deep River neutron monitor) decreased by about 1% after the proton boundary transits and did not have a significant variation associated with the other boundary transits. Thus, a decrease in galactic cosmic ray intensity following boundary transits does not seem to be a necessary condition for a decrease in VAI following boundary transits.

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Effect of humidity on monolayer desorption at the air–water interface

THE effect of atmospheric humidity on the physicochemical properties of lipid monomolecular films at the air–water interface has been thought to be of secondary importance, and has never been studied experimentally. Yet, the physical situation encountered at the air–water interface may be strongly dependent on the humidity in the gas phase^{1,2}. Harkins³, while not specifically investigating this effect, took painstaking measures to ensure saturation of the air above the interface in an effort to suppress any temperature difference between the interior of the aqueous phase and its free surface. Obviously, below the saturation point of the air, the free surface of water is expected to be at a lower temperature than the interior by virtue of the evaporation of liquid. Thus, in some instances, the temperature of the outermost layer of the free water surface was reported to drop by 15°C or more relative to the temperature of the bulk from measurements of the surface tension during evaporation⁴. In other investigations the mean temperature, measured by thermistors, of a thin region immediately below the free surface of a water pool was reported to be several degrees lower than that in the interior during evaporation¹. The data presented here constitute a clear indication of the decisive effect of atmospheric humidity on the kinetics of monolayer desorption from the air–water interface. Figure 1 illustrates the dramatic difference in the observed decrease of the surface pressure of myristic acid monolayers in three different conditions of atmospheric humidity.

Unstable density and temperature gradients may thus be expected along the depth of a water layer undergoing evaporation. The main effect of these gradients is the generation, at a critical value, of the well known convective flows^{5,6} in which liquid at the surface is driven, by buoyancy or surface tension effects, to the interior and replaced by liquid from below. The intensity of these hydrodynamic effects obviously should be a function of the rate of evaporation (or heat flux) and hence, in the case of water pools, of the relative humidity of the air above

the free liquid surface. As the motion of fluid involves exchange of surface liquid for bulk liquid it could, in principle, affect studies of the dynamics of surfactant molecules both along the interface, such as surface diffusion, or across the interface, such as adsorption–desorption phenomena.

The apparatus used in this study consisted of a 1.6-mm deep Teflon trough, a Wilhelmy-type film balance, three thermistor probes for scanning the temperature of both the liquid and gas phases, and small relative humidity probes placed 5 mm above the interface. The entire system was enclosed in a Lucite cabinet, and the experiments were conducted in a controlled environment room. The cabinet was fitted with small pluggable access ports which served two purposes: first, to allow spreading of the monolayer from *n*-hexane through long capillary tubes, thus minimising disturbances of the physical conditions of the system, and second, to allow for adjustment of the humidity levels in the cabinet which also contained moist filter paper. In every instance spreading of the monolayer was accomplished only after complete stabilisation of the temperature and relative humidity. Every effort was made to check for and eliminate experimental artefacts due to film leakage, contact angle hysteresis, and solvent effects.

It is believed that curve *a* in Fig. 1 represents the rate of monolayer desorption in a convection-free aqueous layer (pH 2.0). The increase in the apparent desorption rate at 68% relative humidity (curve *b*) is presumably due to the convective motion in the liquid which causes a more rapid depletion of surfactant molecules from the surface, perhaps even during the spreading process. Interestingly, if the relative humidity is allowed to increase from 72 to 84% (curve *c*), the apparent film pressure first decreases rapidly, due to enhanced surfactant depletion by convection, and then increases gradually as a result of surfactant readsorption which could be aided by the well known suppression of convective motion in liquids by the presence of solvent-free monolayers at the surface. The behaviour depicted also was found to persist for lauric acid monolayers in the liquid-expanded state and, except for curve (*c*), the results for a given surfactant were independent of trough dimensions or construction material.

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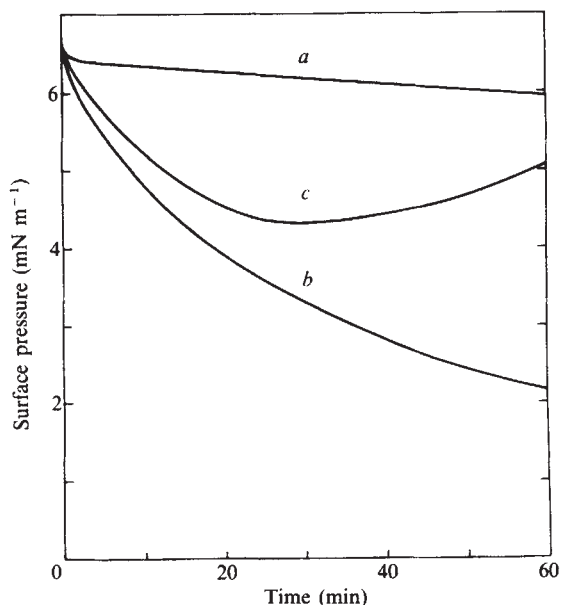


Fig. 1 Monomolecular film behaviour of myristic acid on 0.01M HCl at constant surface area. Relative humidities are 100% (*a*), 68% (*b*) and increasing 72 to 84% (*c*). The surface pressure was measured within 30 s of spreading and compressing the film. Each curve is reproducible to within at least 0.1 mN m⁻¹. Temperature 21.2°C.

Ice elevation map of Queen Maud Land, Antarctica, from balloon altimetry

THE technique and expected coverage of ice elevation mapping using radio altimetry data from the Twerle balloons¹, have already been discussed². We present here preliminary results obtained after analysing 18 balloon traverses, with 1,900 elevation points. A map of the entire continent agrees very well

with existing maps, suggesting the validity of the method. A larger-scale, more detailed map of Queen Maud Land permits better delineation of the drainage systems of that region than has previously been possible.

In Fig. 1 the entire continent is separated into 30×38 km rectangles. The digit printed in the rectangle is the 100-m digit of the unweighted average of all elevation readings obtained in that rectangle (S represents the elevation range of 20–99 m). Contour lines at 500-m intervals are plotted where data coverage permits. Figure 2 is an expanded scale of the 330° – 90° E section, with the balloon trajectories and contour lines every 100 m.

The contour lines on both maps are based on the balloon altimetry data alone. The elevation accuracy, has been shown² to be better than 60 m. The data gathered thus far does indicate a change in that figure of accuracy.

Although the map of the entire continent (Fig. 1) does not show any new features of the general pattern of elevations on the Antarctic ice sheet it does demonstrate the validity of the balloon technique by its close correspondence with standard American and Soviet maps of surface elevation of the Antarctic ice sheet based on data from ground traverses and aircraft flights^{3,4}. An example is the occurrence in central East Antarctica of a closed 4,000 m contour similar in size and shape to that previously observed from Soviet over-snow traverses, showing the good agreement between the data from different sources in this region of maximum elevation above sea level and maximum distance from the coast.

The more detailed map (Fig. 2) is of particular interest from the glaciological standpoint, as Queen Maud Land is a region in which there were very few previous elevation data. Although mapping is not yet complete, there are already some interesting features.

For example, a 3,700-m closed contour appears at about 77° S 40° E, indicating a fourth 'dome' along the central East Antarctic Ridge ('Dome A', 'Dome C', to the east, are already well-known). This feature does not appear on either of the standard Antarctic maps^{3,4}, although it is on a map drawn from the measurements on the Japanese traverse in 1968–69 from Syowa to Plateau Station⁵. The contour in Fig. 2 has been shaped to agree with the Japanese map. However, an additional balloon traverse, only partially analysed, hints that this 3,700 m contour is considerably broader, and may extend as far as 34° E.

On the other hand, the absence of a major dome (higher than the contour interval) centred at about 75° S 12° E, that is shown on the American map³ but not on the Soviet map⁴, is confirmed by the present map. Previous data spacing was wide enough to permit either interpretation.

Of more interest than the absence or presence of domes along the summit ridge may be the indication of valleys inland from several coastal glaciers, showing the convergence of ice flow from substantial drainage basins into those glaciers, and in some cases suggesting the occurrence of ice streams. One such valley, presumably feeding into Recovery Glacier, shows clearly around 81° – 82° S, 0° – 10° W, some 350 km inland. Crevasses as



Fig. 1 Elevation map (polar azimuthal equidistance projection) for the whole Antarctic continent from balloon altimeter data. Small numbers denote the hundreds digit in the elevation figure. Contour interval is 500 m.

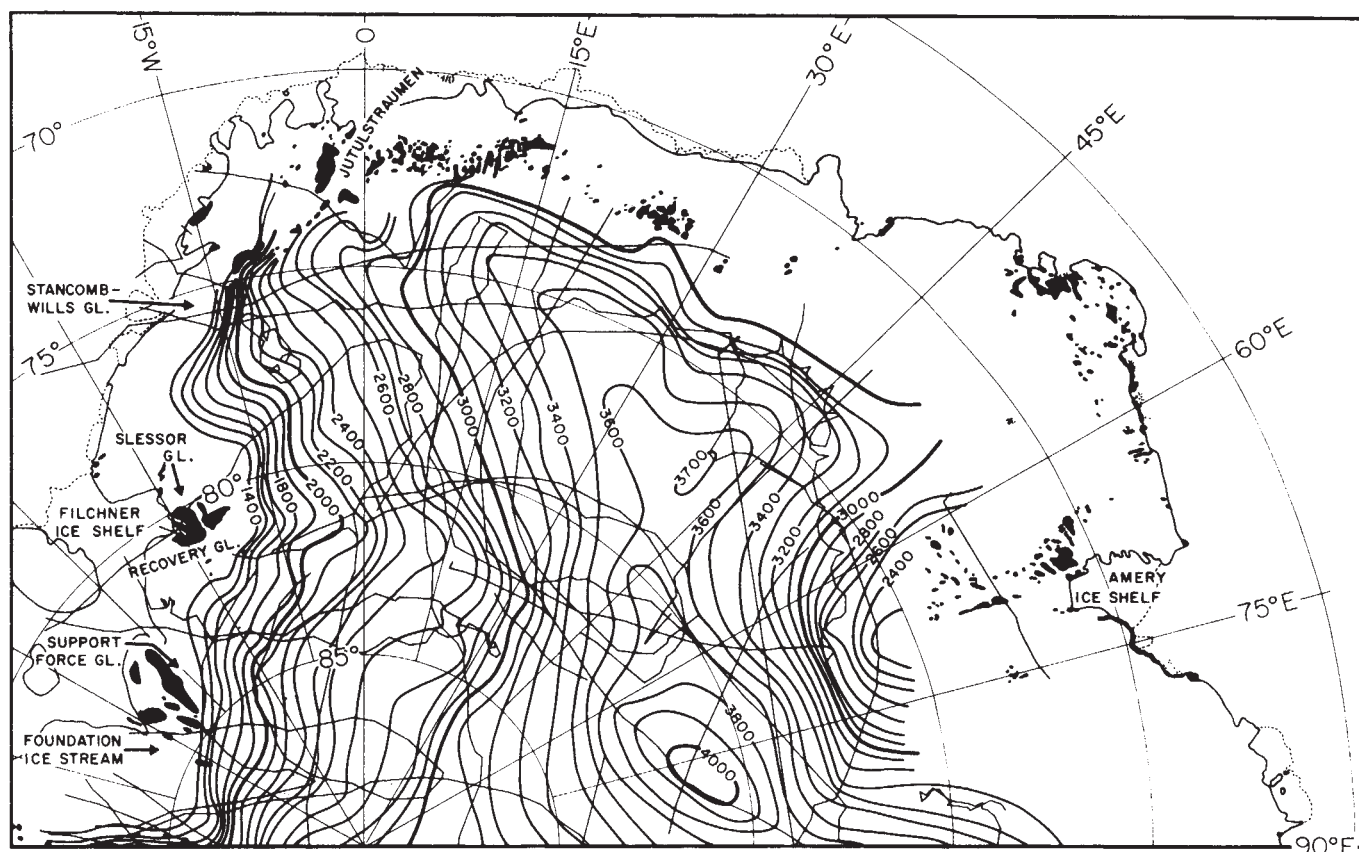


Fig. 2 Elevation map (polar azimuthal equidistance projection) of Queen Maud Land and neighbouring regions. Thin lines are individual balloon tracks. Contour interval is 100 m.

far inland as 83° S 15° E (ref. 6) suggest a deeply penetrating ice stream⁷.

Another valley can be seen around 77.5° S 10° W. Contour lines have been interpolated between rather widely-spaced balloon tracks on the assumption that this valley is associated with Slessor Glacier. Although that is not definite, the shape of the Coats Land coast necessitates the pronounced westward bowing of the contours north-east of Slessor Glacier (Fig. 2) unless the ice sheet profile in that region is most exceptionally flat. Furthermore, massive outflow through the Recovery Glacier-Slessor Glacier system is consistent with the high movement rates of the Filchner Ice Shelf front around 40° W^{8,9}. As drawn, this drainage system is the largest in East Antarctica, exceeding that feeding the Amery Ice Shelf by ~20%.

Stancomb-Wills Glacier (Dalglish Ice Stream) is very fast-moving—the ice shelf it feeds is advancing at almost 1.5 km yr⁻¹ (ref. 10). In the light of this, the drainage area that seems to feed it is surprisingly small.

A clear indentation of contours, reflecting a major region of outflow through the mountains of New Schwabenland by Jutulstraumen⁹ appears just north-east of 75° S 0° E. This valley shows good agreement with elevations from the single traverse line in the region, that of the Norwegian-British-Swedish Antarctic Expedition, 1949–52.

In contrast, there appears from fairly good coverage to be little contour line indentation inland of the Pensacola Mountains, where one might otherwise suspect concentrated drainage through either Support Force Glacier, or Foundation Ice Stream, or both. The total area of the drainage basin feeding the southern Filchner Ice Shelf, formerly thought to be the main source of the ice shelf input¹¹, is now seen to be only about half as large as the Recovery Glacier-Slessor Glacier system.

Work on the mapping of the Antarctic ice sheet from the

balloon data continues, and several of the more speculative features mentioned here clearly require closer examination as the data coverage increases. Nevertheless, the map presented here for Queen Maud Land is already substantially more detailed than any previously available.

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Depth of curie temperature in continental shields: a compositional boundary?

MUCH attention is being given to estimating the depth to the bottom of the crustal magnetised layer. The latter can be interpreted as the surface where ferrimagnetic minerals, which are responsible for the magnetisation of crustal rocks, reach their Curie temperatures 1–5. The depth of this surface is therefore temperature dependent and it can be used to locate thermal anomalies within the continental crust. One application would be in the search for shallow intercrustal heat sources (such as large magmatic reservoirs or cooling plutons) which would be of economic interest as natural energy sources^{3,6}. In all these works the bottom of the crustal magnetised layer is assumed to correspond to an isotherm whose temperature is approximately the Curie temperature of pure magnetite (580 °C). Haggerty⁷ cautioned against assuming a single Curie temperature for the continental crust and pointed out that Curie temperatures as low as 300 °C may exist in the crust because of the low temperature oxidation of titaniferous magnetite. Furthermore, he suggested that depths of Curie temperatures greater than 20 km may be due to serpentinisation of mafic and ultramafic components of the continental lower crust with consequent formation of Fe–Co–Ni alloys with Curie temperatures in the range 620–1,100 °C. We show here that depths of Curie temperatures computed for continental areas characterised by normal and low heat flow cannot be interpreted as isothermal surfaces with temperatures at or above 580 °C. We suggest that the bottom of the crustal magnetised layer may correspond to vertical compositional changes within the crust.

This idea occurred to us while we were interpreting the results of the computation of the depth to the bottom of the crustal magnetised layer in southeastern Minas Gerais, Brazil⁸. This is a Precambrian area where extensive outcrops of high grade metamorphic rocks occur. This metamorphic basement was formed 2,000–2,600 Myr ago⁹ and it is locally covered by metasediments of Upper Precambrian age and sediments formed during the last orogenic event which affected this area, the Brazilian orogenesis (600–650 Myr)^{9,10}. A crustal model was computed for a part of this area from gravity data¹¹ also using the information obtained from an unreversed deep refraction profile¹². This model indicates a normal crustal thickness of

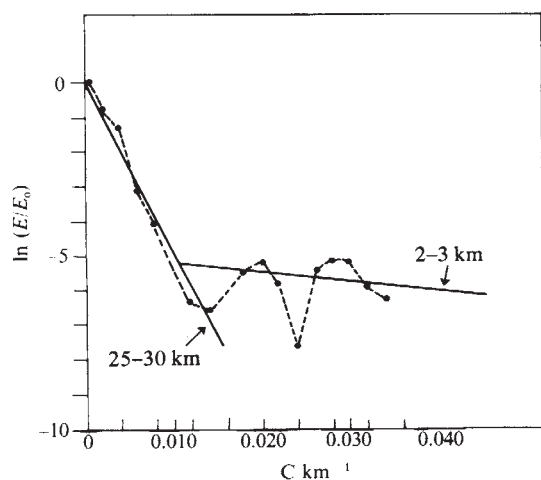


Fig. 1 Power spectrum of the total intensity anomaly field in southeastern Minas Gerais, Brazil. Continuous lines are regression lines relative to depth of anomalies at 25–30 km and 2–3 km, respectively.

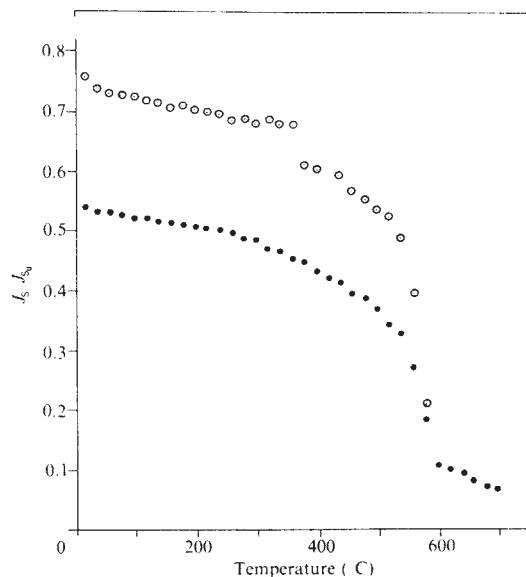


Fig. 2 Saturation magnetisation plotted against temperatures for two acidic charnockites outcropping along the coast of Sao Paulo State, Brazil. Chemical and petrological data about these rocks are available²⁰. Measurements were carried out on the Curie balance of the Istituto di Geologia e Geofisica of the University of Napoli, Italy²⁷. ○, MP003; ○, MP005.

38–40 km with local thickening to 45 km (see Fig. 3). Furthermore, predominantly acidic rocks seem to be the crustal constituents at least to a depth of 22–25 km. The gravity derived model requires a lower crustal layer with density between 2.8 and 3.3 g cm⁻³. If a density of 3.0 g cm⁻³ is assumed, the normal depth to the top of this layer is about 20–22 km, with local thickening to 25 km. Seismic data¹² indicate a continuous increase of P-wave velocities in the lowermost 15 km of crust, with an average velocity of 7.4 km s⁻¹. Preliminary heat flow data in this area are in the range of 40–50 mW m⁻² with temperature gradients in the range 10–16 °C km⁻¹ (ref. 13).

The depth to the bottom of the magnetised crustal layer was computed from a very detailed aeromagnetic survey which was carried out in 1973 by the German company Prakla and the Brazilian Companhia de Pesquisa de Recursos Minerais¹⁴. The map of the total intensity anomalies was averaged on a 10 × 10 km grid and was then submitted to Fourier analysis. A typical power spectrum is given in Fig. 1. The decay of the energy content with frequency¹⁵ indicates a distribution of anomaly sources at 25–30 km at a depth of 2–3 km. A low pass filter with cut-off at 0.013 cycles per km (wavelength 80 km) was applied. The resultant residual anomalies were inverted by using two-dimensional polygonal bodies of uniform magnetisation whose upper surface corresponds to the ground level and the depth and morphology of its bottom is computed by iterative procedures which compare the theoretical field produced by the model with the observed field. The computed depths are in the range 22–27 km⁸.

These depths are in the range of 'normal' depths reported in the literature for areas of normal and slightly higher than normal heat flow^{4,5}. The average depth of Curie temperature in continental areas was estimated to be about 20 km (ref. 1). A linear extrapolation of shallow temperature gradients would actually predict temperatures higher than 500 °C at a depth of 20 km in areas of normal heat flow. However, a constant temperature gradient within the continental crust is not allowed by the energy balance between the measured heat flow and the radiogenic heat production determined on rocks which are believed to be the main constituents of the upper continental crust. Heat flows and radiogenic heat productions measured in

continental areas require a strong depletion of radioactive elements with depth¹⁶⁻¹⁸. This is also supported by geochemical considerations¹⁹. The main consequence of an upward concentration of radioactive elements is that the temperature gradient decreases with depth. Figure 3 shows the highest temperatures which can be reached within the crust of a normal heat flow area¹⁷. This temperature profile was calculated by assuming a stepwise distribution of radioactive elements, that is, a 5-km thick upper crustal layer with concentrations typical of acidic intrusive rocks and a 35-km layer with concentrations typical of high grade metamorphic rocks¹⁷. The highest temperatures which can be reached at 20–25 km of depth are about 250–300 °C. Temperatures above 500 °C can be reached only below the crust–upper mantle boundary. Only in areas with heat flow higher than 80 mW.m⁻², temperatures of 500–580 °C are possible at a depth of about 20 km. Different models of depletion of radioactive elements with depth do not significantly change these conclusions^{18,20}. Therefore the bottom of the crustal magnetised layer computed for areas of normal and low heat flow cannot represent an isothermal surface at 500–580 °C.

Geophysical models of the continental crust of typical Precambrian areas, such as the Canadian Shield²¹ or the Baltic Shield²², are similar to that inferred for Minas Gerais (Fig. 3). P-wave velocities of 6.1–6.3 km s⁻¹ and densities of 2.8–2.9 g cm⁻³ are determined down to a depth of 15–20 km. These characteristics are consistent with the occurrence of rocks of acidic composition and low to high (granulite facies) metamorphic grade²³. P-wave velocities of 6.8–7.4 km s⁻¹ and density ~3.0 g cm⁻³ are usually inferred for the lower continental crust. Some geophysicists²⁴ believe that higher seismic velocities and densities of the deep continental crust are due to a progressive increase of metamorphic grade. However, P-wave velocities higher than 6.8 km s⁻¹ have been measured in granulitic facies rocks with at least 40% pyroxene plus hornblende at pressures of 4–10 kbar²⁵. Although the available seismic data indicate a very complex structure for the continental lower crust²⁵, the increase of metamorphic grade must be accompanied by a change of chemical composition toward intermediate or mafic in order to fit the seismic velocities²³.

Curie temperatures of acidic rocks are unlikely to be lower than 500 °C. It is well known that plutonic rocks and lavas of acidic composition contain almost pure magnetite as the main ferrimagnetic phase²⁶. Curie temperatures above 500 °C have also been measured in acidic rocks which are stable in the lower continental crust. Saturation magnetisation versus temperature curves for two charnockites outcropping along the coast of Sao Paulo State, Brazil, are reported in Fig. 2. One curve has a clear Curie temperature at 580 °C, indicating that pure magnetite is the main ferrimagnetic phase. The other sample is characterised by a more complex curve, with a tail to about 700 °C. This is probably a consequence of low temperature oxidation of the rock with resulting production of maghemite (Curie temperature 545 °C), which, being unstable at temperatures between 250

and 550 °C, tends to transform into hematite (Curie temperature 860 °C). High grade metamorphism of acidic rocks is hence unlikely to give rise to low Curie temperature ferrimagnetic phases.

Ferrimagnetic phases contained in alkali-basaltic and andesitic rocks are intermediate members of the ulvospinel-magnetite solid solution series, with Curie temperatures in the range 100–300 °C^{26,27}. No experimental data are available on Curie temperatures of high grade metamorphic rocks of intermediate to mafic composition. However, in this case magnetisation of the intermediate members of the ulvospine-magnetite solid solution series would produce Curie temperatures in the range 300–450 °C⁷. Curie temperatures of rocks of intermediate (andesitic) or mafic composition are in the range of possible crustal temperatures of Precambrian areas. The Curie temperature at depth can therefore correspond to the surface where a change of chemical composition of crustal rocks occurs.

Figure 3 shows this correspondence with reference to the crustal model of southeastern Minas Gerais, Brazil. The Curie temperature is assumed to be 580 °C in the upper crust and about 200 °C in the lower crust. The bottom of the magnetised layer (dashed area) is slightly below the top of the lower crustal layer and corresponds to a compositional change of the crust towards more mafic terms.

If this interpretation is correct, the depth of Curie temperature is not sensitive to small variations in the thermal state of the crust, but only to the composition of rocks at that depth. The depth of Curie temperature can be used therefore as a marker of vertical changes in chemical composition of the continental crust in normal heat flow areas.

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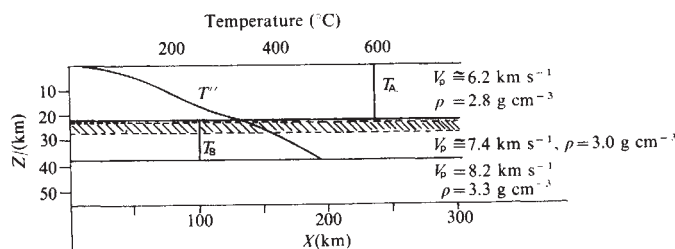


Fig. 3 Geophysical model of the continental crust in southeastern Minas Gerais from gravity¹¹, seismic¹², heat flow¹³ and aeromagnetic data. Curie temperature of crustal rocks at depth 0–22 km (T_A); at depth 22–38 km (T_B); T'' , maximum allowable temperature in shield areas; hatched area is the bottom of the magnetised layer.

Palaeo-oceanography of the post-glacial Eastern Mediterranean

DURING the late Pleistocene the hydrography of the Eastern Mediterranean underwent severe changes in response to climatic fluctuations. Among the most striking of these changes were periods of deep water stagnation. Episodes of oxygen depletion are indicated by the presence of layers of dark sapropelitic muds enriched in organic carbon, devoid of the remains of benthic organisms¹⁻⁴. While stagnant conditions prevailed in the deep basin, the water column above 1,000–800 m remained ventilated, and cores raised above this depth do not contain sapropels⁴⁻⁶. Several theories have been proposed to explain the past stagnations. They all use a mechanism which stabilises the water column by forming a steep density gradient in the upper waters. In this way the vertical mixing, and hence the ventilation, are retarded, oxygen is depleted in the deep water and stagnation commences. Olauson's theory⁷, whereby influx of freshwater leads to reduced surface density and the interruption of normal vertical circulation, is the most widely accepted at present. This theory is supported by recent studies⁸⁻¹⁰, which use measurements of oxygen isotope composition in shells of planktonic foraminifera. These studies demonstrate that many of the sapropels correspond to $\delta^{18}\text{O}$ minima, suggesting freshwater influx, and thus a reduced upper water salinity at the time of the sapropel formation. Moreover, the epipelagic species record greater depletion of ^{18}O than the mesopelagic species, indicative of the establishment of salinity and density gradient in the upper waters¹⁰. Using a similar approach we have attempted to investigate in detail the hydrographic changes associated with the deposition of the most recent sapropel, which took place about 7,000–9,000 yr BP⁶.

Core GA32 from the continental slope off Israel—an area of very rapid accumulation of Nile derived sediments¹¹⁻¹³—was sampled every 10 cm, which represent intervals of about 300 yr. The epipelagic planktonic species *Globigerinoides ruber* and the benthonic species *Uvigerina mediterranea* were chosen for analyses of $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$. Being a shallow core, GA32 does not contain any sapropels. However, determination of the core interval corresponding to the time of the last sapropel deposition in the deep cores is straightforward when the $\delta^{18}\text{O}$ minimum of *G. ruber* (230–320 cm, Fig. 1) is used to correlate other core records such as RC9–181 and TR172–22^{8,9}. A time scale based on this correlation and on an assumption of constant accumulation rate is used in Fig. 1. Note that this scale should be used only as a rough guide because the accumulation rate of the core might not be uniform¹².

In general the benthonic species is enriched in ^{18}O relative to the planktonic species. At present this difference is primarily due to two effects. The oxygen isotopic fractionation between the calcitic shells and the seawater is temperature dependent, therefore at 590 m ($\sim 14^\circ\text{C}$) ^{18}O becomes more enriched in the shells than at the surface ($18\text{--}28^\circ\text{C}$). In addition, nonequilibrium isotopic fractionation due to vital effects¹⁴⁻¹⁶ would tend to increase the difference between *G. ruber* and *U. mediterranea*.

Assuming that *G. ruber* has not undergone significant environmental adjustments such as a change in its depth habitat or a change in the season of optimal shell production, and that the isotopic fractionation between the shells of both species and the seawater has remained nearly constant during the past 12,000 yr, hydrographic conditions can be inferred from the isotopic variations.

Rapid ^{18}O depletion in both the planktonic and the benthonic species is registered in the lower part of GA32. This depletion corresponds to a transition from cool to warmer faunal composition in the area which was attributed to the ending of the last Glacial^{12,17}. Nevertheless, it seems that temperature alone would not account for all the isotopic change, as the faunal change over the same period is small, and temperature estimates

based on this change suggest that the temperature rise did not exceed $2\text{--}3^\circ\text{C}$ ⁹. Thus the major part of the isotopic change ($\sim 1.5\%$) has to be accounted for by an influx of freshwater. It has been suggested that glacier melt-water was the major source of this influx¹⁰, but increased precipitation at about the same time cannot be neglected⁹. In either case, the freshwater input would result in a considerable reduction of the surface salinity ($2\text{--}3\%$)¹⁰.

It is significant that the salinity changes on the bottom lag behind the surface changes. This lag is explained by a very slow mixing of the low salinity surface layer. For $\sim 1,500$ yr there was enough freshwater input to cope with the vertical mixing, and throughout this period stable stratification of the upper water column was maintained, as is evident from the difference between the isotopic composition of the surface dwelling and bottom dwelling foraminifera (Fig. 1). Following a brief reduction in the steepness of the density gradient ($\sim 7,500$ yr BP), stable water-mass stratification was re-established for several hundred years. It seems then that the supply of freshwater was not steady throughout the deposition of the last sapropel. Further indication of this is found in cores from the Adriatic Sea, and also in some deep cores from the Levantine basin, where the last sapropel is interrupted by a short period of ventilated bottom water^{4,8}. However, in many deep cores from the Eastern Mediterranean there is no such interruption. This might indicate that only part of the basin was ventilated in this short period, or that the rate of accumulation of many cores is too slow and a short event cannot be recorded.

Following the sapropel time, the $\delta^{18}\text{O}$ curves register only small fluctuations, and thus indicate relatively stable climatic and hydrographic conditions. However, in the period 4,000–3,000 yr BP the planktonic species was enriched in ^{18}O , and the difference between the lower and upper waters is reduced. This might indicate a more arid climate during this period.

Unlike the $\delta^{18}\text{O}$ in the foraminiferal shells, the $\delta^{13}\text{C}$ is not directly influenced by changes in the evaporation–precipitation budget, and its temperature dependence is small. Rather, it depends mainly on the isotopic composition of the dissolved CO_2 in the seawater, and on some biogenic effects which influence the isotopic fractionation^{15,16,18,19}.

The $\delta^{13}\text{C}$ in *G. ruber* from GA32 is 1.4% on the average and

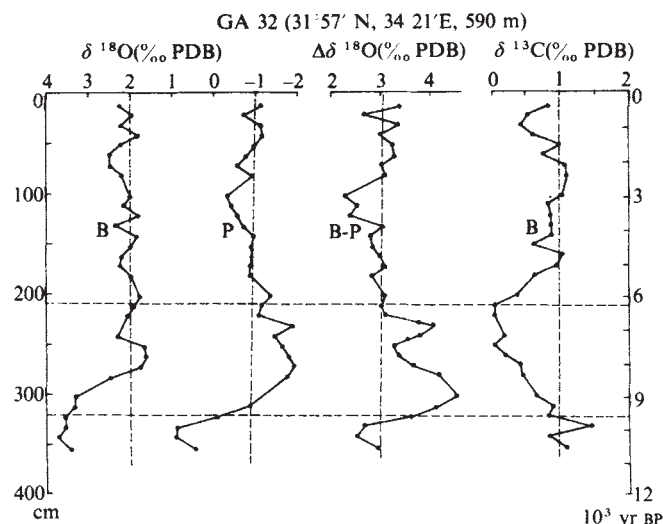


Fig. 1 $\delta^{18}\text{O}$ curves of the benthonic (B) species *Uvigerina mediterranea*, the planktonic (P) species *Globigerinoides ruber*, the difference ($\Delta\delta^{18}\text{O}$) between the planktonic and the benthonic species, and $\delta^{13}\text{C}$ curve of the benthonic species. Standard procedures were followed in the isotopic analyses²³, and 20 specimens of the planktonic species and five specimens of the benthonic were analysed on a Micromass 602C mass-spectrometer. Conversion to PDB was done through the CalTech standard PDB IV.

varies by about $\pm 0.5\%$. However, its changing values do not show an understandable trend. In this respect the record of GA32 is no exception to other such records of planktonic foraminifera from the Eastern Mediterranean^{9,20}. The $\delta^{13}\text{C}$ curve obtained for the benthonic species in turn shows a distinct minimum centred around 7,000 yr BP. This minimum probably resulted from increased proportion of dissolved carbon dioxide which originated from oxidation of organic matter. A similar process leads today to the formation of the $\delta^{13}\text{C}$ minimum which is closely associated with the oceanic oxygen minimum layer^{20,22}. Thus during periods of oxygen depletion in the deep parts of the Eastern Mediterranean basin, the $\delta^{13}\text{C}$ of the dissolved CO_2 decreased. The layer of the low oxygen content and low $\delta^{13}\text{C}$ propagated slowly upwards, and thus the minimum values reached the depth of a few hundred metres only 1,500 yr after the initiation of increased freshwater influx. Once this influx decreased and the ventilation resumed, the carbon isotope composition returned back to the pre-sapropel values. It is also possible that the ^{13}C -depleted carbon dioxide was not derived from direct oxidation of organic matter in the water column, but from oxidation of organic remains in the sediment. The latter process is slower, and might explain the slow response of the $\delta^{13}\text{C}$ to the changes in surface conditions.

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Evidence against Fisher's theory of dominance

FISHER'S theory of the evolution of dominance is based on the fact that heterozygotes for rare deleterious alleles, maintained by recurrent mutation from the wild-type allele at the locus, greatly outnumber homozygotes in a random-mating population at equilibrium¹. The fitness of a heterozygote carrying a mutant and the wild-type allele can be written as $1 - hs$ ($1 \geq h \geq 0$), compared with fitnesses of 1 and $1 - s$ for wild-type and mutant homozygotes respectively. Fisher postulated that mutant alleles were originally semidominant, so that $h > 0$. He explained the fact that most mutations are observed to be

recessive or nearly recessive to wild type as the result of selection for modifier genes which increase the fitness of the heterozygotes alone. He pointed out that selection to improve the fitness of the mutant homozygotes is likely to be relatively ineffective because of their rarity compared with the heterozygotes, and also because the presence of the wild-type allele in heterozygotes renders them more amenable to selection towards the wild type. Using this theory, the level of dominance of the mutant alleles, h , is gradually reduced towards zero, while s remains constant. Wright² criticised this because the heterozygotes for mutant alleles are so rare that the intensity of selection on a gene which modifies h is of the same order as the rate of mutation, u , to deleterious alleles at the locus undergoing dominance modification. He suggested that such a weak selection pressure is unlikely to overcome the effects of random genetic drift, or the selective consequences of pleiotropic effects of the dominance modifier. Later theoretical work^{3,4} has confirmed Wright's conclusion about the intensity of selection on dominance modifiers, so that many geneticists have come to doubt the validity of Fisher's theory³. The principle of dominance modification when applied to balanced polymorphisms (for example, Batesian mimicry⁵) is not, of course, subject to this objection and is generally accepted. Fisher¹, however, believed that the recessivity of mutant genes demonstrated the ability of even a minute selection pressure to cause significant evolutionary change. The issue cannot be settled by purely theoretical considerations, and it is evident that some empirical evidence capable of providing a critical test is required. (Demonstrations that dominance can be modified by artificial selection^{6,7}, although of great interest, are not relevant to the question of the efficacy of selection pressures as small as those postulated in Fisher's theory.) This paper points out recent experimental evidence on the heterozygous effects of mutations affecting viability in *Drosophila*⁸ that seems to be inconsistent with Fisher's theory, and also looks at two alternative theories proposed by Haldane⁹.

Fisher's theory implies that the intensity of selection on a modifier of dominance depends only on the mutation rate and the extent to which the level of dominance is affected by the modifier, and is independent of the homozygous effect of the mutant alleles^{2,3}. The reason for this can be seen as follows. The increment in fitness to the heterozygote after reducing h by a quantity Δh is $s\Delta h$. But the frequency of the heterozygotes is approximately $2u/hs$ ^{1,2}, so that the net intensity of selection on a modifier with effect Δh is $2u\Delta h/h$, which is independent of s . This argument tacitly assumes that linkage disequilibrium between the modifier and the primary locus can be neglected. Exact numerical computations of population trajectories suggest that this assumption gives an excellent approximation^{3,10}, except when dominance modification has proceeded so far that the mutant alleles have been rendered almost recessive. A similar conclusion about the lack of dependence of the strength of selection for dominance modification on s is true for the two models suggested by Haldane⁹. The first of these involves the selection of wild-type isoalleles which are more dominant than the original wild type over the mutant alleles at a locus. The second involves selection for modifiers that strengthen the dominance of a favourable gene which is in the process of replacing an alternative allele. All three models predict that dominance should be reduced at about the same rate for genes with drastic effects on fitness as for genes with minor effects, so that no relationship between h and s should be observable.

The extensive literature on the heterozygous effects of deleterious genes affecting egg-to-adult viability in *Drosophila* was recently reviewed by Simmons and Crow⁸. It seems clear that newly-arisen spontaneous lethal mutations lower viability by 2–4% when heterozygous, giving an average h value of about 0.03 (ref. 11). The evidence is less clear for lethals derived from natural populations. Some studies have suggested heterotic effects of lethals^{12–14}, although most experiments seem to show small, deleterious effects of heterozygous lethals⁸. There is no indication, however, of anything but a small h value, of the order

of a few per cent, so lethals are usually nearly recessive in their effects on viability. This contrasts with findings on mildly deleterious genes. The evidence suggests an average h value of about 0.35 for newly-arisen, spontaneous mutations of small homozygous effect^{15,16}, and about 0.2–0.3 for mildly deleterious mutations carried on chromosomes extracted from natural populations^{8,16}. Although there are some technical problems associated with these estimates^{8,17,18}, the biases are in the direction of underestimating h for mildly deleterious genes. The experimental evidence therefore seems to contradict the predictions of the above models for the evolution of dominance, as h is found to be negatively correlated with s .

In contrast, Wright's² physiological theory of dominance predicts this type of relationship between h and s (ref. 19). This theory assumes that the function of most gene loci is to specify the structure of enzymes, and that most mutations cause a reduction in enzyme activity. If the wild-type enzyme is so active that the concentration of substrate rather than enzyme is rate-limiting for the enzyme-catalysed reaction in wild-type homozygotes, then the rate of the reaction is likely to show a law of diminishing returns as a function of the concentration or activity of the enzyme. This leads to the property that an allele that only slightly reduces enzyme activity is more nearly semi-dominant in combination with a wild-type allele conferring full activity, than an allele which reduces activity to near zero. If there is a relationship between fitness and the rate of production of the end product of the pathway in which the enzyme is involved, alleles with a high s will have a low h , and vice versa¹⁹. This theory requires that the wild-type enzyme has such a high activity that halving the concentration of enzyme produces only a small reduction in the reaction rate². Plunkett²⁰ and Muller²¹ have proposed models in which selection for buffering of the wild-type homozygotes provides an explanation for this apparently unnecessarily high efficiency of the wild-type enzyme.

In summary, the observation that h is negatively correlated with s for viability mutations of *Drosophila* suggests that either dominance of the wild-type allele is a purely biochemical phenomenon and has not been selected for, or that it has evolved by mechanisms other than those suggested by Fisher¹ and Haldane⁹.

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Delayed implantation is under environmental control in the badger (*Meles meles* L.)

IN the wild state, the European badger (*Meles meles* L.) becomes pregnant once a year during a post-partum oestrus in February or March¹. Fertilisation is followed, at the blastocyst stage of development, by a prolonged embryonic diapause which ends, when implantation occurs, in December or January. Implantation is thus delayed for 10 months² and it may be assumed, in accordance with Fries's hypothesis³, that the resumption of development in the winter is a genetically determined and adaptive phenomenon which results in birth during the spring when conditions for the young are most favourable. This pattern suggests that the time of implantation is related to some environmental factor such as photoperiod or temperature. To test this hypothesis, wild female badgers were captured in their set after fecundation and maintained during summer in conditions of 'artificial winter'. We report here that in such conditions implantation, gestation and birth can be brought forward 4 months.

Six females were kept at +5 °C in short photoperiod (10 h light: 14 h dark) from July to September. Lighting was provided by fluorescent lamps and the mean intensity of light was about 300 lx. Females were bled once a week and progesterone in plasma was determined, using a radioimmunoassay previously described⁴, to show the changing level of luteal secretion during the period of observation. In natural conditions of daylight and temperature, embryonic diapause in the badger is characterised by a low level of progesterone, the mean not exceeding 6.51 ± 1.60 ng ml⁻¹ (ref. 4). In contrast, just before implantation at the end of December or in January we have found that the level reaches the highest mean value achieved after fertilisation (16.9 ± 2.39 ng ml⁻¹) (ref. 4). When the photoperiod and temperature were decreased experimentally in July, there was a sharp increase in plasma progesterone to peak values (18 ± 0.91 ng ml⁻¹) similarly observed at implantation in January (20.16 ± 1.58 ng ml⁻¹). Experimental females whelped in early September, with plasma progesterone levels during gestation similar to those characterising pregnancy in animals kept in normal conditions of light and temperature (Fig. 1). The litters born in September 1978, that is during the summer, would have

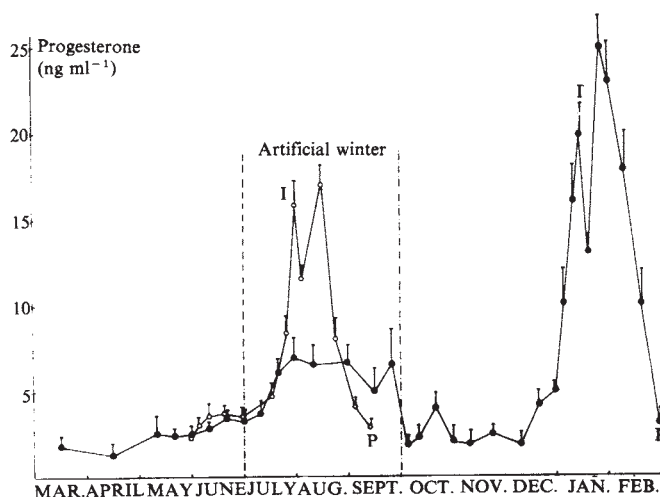


Fig. 1 ●, Progesterone profile during pregnancy in five control badgers (natural daylight and temperature conditions, mean $\pm$ s.e.m.). ○, Progesterone profile in six experimental females subjected to "artificial winter" (mean $\pm$ s.e.m.). With environmental parameters: daylight 10 h, temperature +5 °C unchanged from the beginning of experiment. I, implantation; P, parturition.

been dropped in the following spring had the animals not been exposed to our experimental conditions.

Many mammals undergo well defined annual cycles of sexual activity, and it has been demonstrated in several species that the cycle is influenced by photoperiod^{5,6}. In the mink⁷⁻⁹ and in the marten¹⁰ gestation becomes shorter when the photoperiod is increased artificially.

The experiment described here shows that full luteal function can be stimulated in the badger by manipulating the photoperiod and temperature, and it also suggests that in artificial conditions badgers could have two litters a year. The result conforms with our hypothesis that the hormonal changes which control implantation in the badger are activated by what we have called an 'impulsive factor'¹¹. Further experiments are underway to determine the relative significance of temperature and light in the operation of this factor.

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Pattern-selective adaptation in visual cortical neurones

PROLONGED viewing of a grating pattern produces striking 'after-effects', involving changes in the detectability, apparent size, orientation and contrast of subsequently viewed gratings¹⁻³. Studies of perceptual after-effects have been used to infer properties of neurones in the human visual cortex^{2,4,5} similar to those pattern-selective neurones whose sensitivities have been directly measured in the visual cortex of cats and monkeys^{6,7}. Such inferences are based on two assumptions: first, that perceptual changes result from changes in the distribution of activity within the responding population of neurones; second, that the effect of adaptation on each neurone of the population is to reduce its sensitivity uniformly to all stimuli. The experimental results reported here support the first but challenge the second assumption, as they show that after adaptation to a particular grating the sensitivity of a single neurone to that grating may be reduced more than its sensitivity to other gratings.

We recorded the activity of single neurones in the portion of the striate cortex representing the central visual field in young adult cats anaesthetised with nitrous oxide, using methods described in detail elsewhere⁸. The after-effects of prolonged stimulation by grating patterns were examined in 20 cells in five cats. Initially, we explored the receptive fields of each neurone with bars, edges and spots, and as a result classified them as simple, complex or hypercomplex^{6,7}. The face of a display

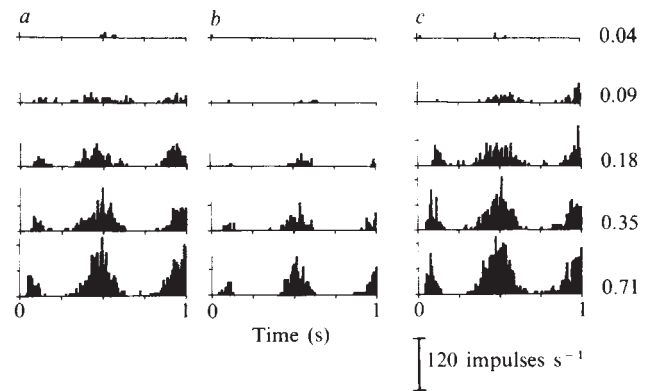


Fig. 1 Responses of a simple cell to gratings of different contrast, obtained before (a), during (b) and after (c) a period of adaptation. a Shows averaged responses to gratings of 0.75 c deg^{-1} , moving at 2 Hz in the optimal direction, presented at the five contrasts indicated on the right. Each grating was presented 20 times, each time for two stimulus cycles; each histogram thus shows the response to the passage of two bars of the grating across the receptive field. b Shows the responses to the test stimuli while the neurone was fully adapted to the same grating presented at high contrast (0.71). Each test stimulus was preceded by a presentation of the adapting stimulus. c Shows responses to the same test stimuli, measured in an experiment begun 12 min after the end of the adaptation experiment.

oscilloscope, which subtended a circle 10° in diameter at the cat's eye, was then centred on the receptive field (in the dominant eye); the other eye was covered.

The luminance of the oscilloscope screen (which was kept at a space average of 6 cd m^{-2} throughout) could be spatially modulated by signals from a computer to produce sinusoidal gratings. These are patterns of light and dark bars whose luminance profile perpendicular to the bars is a sine wave; their spatial frequency is the number of cycles that subtend one degree of visual angle, and their contrast is the difference between the maximum and minimum luminance divided by twice the mean luminance. All the gratings moved steadily in a direction orthogonal to their bars at 2 Hz (so that two cycles crossed any point on the screen in 1 s). The computer controlled the orientation, direction of movement, spatial frequency and contrast of the grating, and also, within the limits of the 10° field, its spatial extent. While presenting grating patterns, the computer counted action potentials and compiled them into average response histograms; these were saved on disk for later analysis.

For each neurone we first established the spatial frequency, orientation, extent and direction of movement of the optimal grating; then, using optimal stimuli, we measured the relationship between response and stimulus contrast. Figure 1a shows the results of a set of such measurements. In this and all other experiments, the stimuli were presented in blocks. Within a block, each stimulus was presented once for 1 s, during which two cycles of the grating crossed the receptive field. The order of presentation within a block was random and different within each block, of which there were normally 20 in an experiment.

The contrast-response relationship was then determined again, while the neurone was fully adapted to a high-contrast grating. Each block of this 'adaptation' experiment began with a 32- or 64-s presentation of the adapting grating; each test stimulus after the first was preceded by a 4- or 8-s topping-up presentation of the adapting stimulus². Figure 1b shows the results of an experiment in which the adapting stimulus was of optimum spatial frequency and orientation. A comparison of these responses with their counterparts in Fig. 1a shows clearly that prolonged stimulation by a high-contrast grating greatly reduced the strength of responses. In separate experiments we established that the initial period of adaptation used was

sufficient to produce the maximum reduction in sensitivity, and that the topping-up period was sufficient to maintain it.

Figure 1c shows the results of measurements begun 12 min after adaptation, by which time responses had fully recovered. In all cases, except three where the unit was lost, we verified that within 5–15 min after the end of adaptation, responses were similar to those observed before adaptation.

Figure 2A shows the full results of this experiment, which was carried out on a simple cell, and Fig. 2B shows the results of a similar experiment on a complex cell. For both neurones, Fig. 2 shows the contrast-response relationship measured before adaptation (a), while the cell was fully adapted (b) and after a period of recovery (c). For both neurones, adaptation raised the contrast required to produce a given response by a factor of about four, and recovery returned it to very nearly its former value. Although the changes brought about by adaptation are apparently very similar in form for the two units when expressed in this way, the extent to which the response to a grating of fixed contrast is reduced by adaptation may be a poor guide to the extent of this adaptation. Thus, in the case of the complex cell, adaptation produces a much smaller reduction in the response to high-contrast stimuli than it does to ones of lower contrast. Increase in the contrast required to produce a given response (that is, reduction in sensitivity) seems to be the appropriate measure of adaptation.

To ascertain how great a change in sensitivity is brought about by an adapting stimulus, one must compare responses to stimuli of different contrasts presented before, during and after adaptation. Adaptation brought about clear reductions in sensitivity in 19 of the 20 neurones we studied; the remaining unit, a

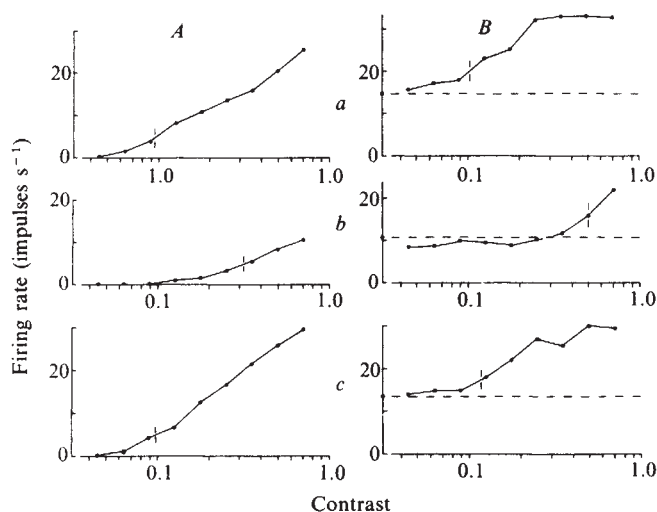


Fig. 2 The effect of adaptation on contrast-response relations. **A**, Results of the experiment partially illustrated in Fig. 1. **a** Shows the contrast-response relationship measured before adaptation, **b** shows the relationship while the cell was fully adapted, and **c** shows it after a 12-min recovery period. **B**, Results of a similar experiment on a complex cell; the spatial frequency of all gratings was 2.0 c deg^{-1} , and they moved at 2 Hz . Horizontal dashed lines indicate spontaneous activity. As in **A**, the three graphs show measurements made before (**a**), during (**b**) and after (**c**) adaptation. The vertical bar on each graph indicates the contrast necessary to elicit a criterion response of 5 impulses s^{-1} above spontaneous firing. Comparison of these criterion contrasts provides a useful index of adaptation; within limits, the choice of criterion does not strongly affect the index obtained. Here and in Fig. 3, the response plotted is the average discharge rate elicited by the stimulus. This is the natural measure of the response of complex cells, which do not give modulated responses to most gratings; simple cells do give modulated responses but as they generally lack maintained discharge, measures of modulated or unmodulated responses are very well correlated (see Fig. 1).

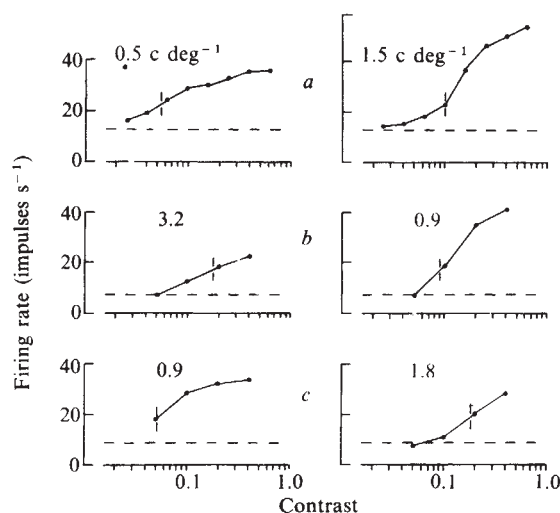


Fig. 3 The spatial frequency selective nature of adaptation. In these experiments, on a complex cell, test gratings of 0.5 and 1.5 c deg^{-1} were used. **a** Shows the contrast-response relationship for both spatial frequencies, measured before adaptation. **b** Shows the results of an adaptation experiment in which the adapting grating had a contrast of 0.71 and a spatial frequency of 0.5 c deg^{-1} . **c** Shows the results of a similar experiment in which the adapting grating had a contrast of 0.71 and a spatial frequency of 1.5 c deg^{-1} . The vertical bars on each graph, as in Fig. 2, indicate the contrast needed to elicit a criterion response (here, $10 \text{ impulses s}^{-1}$ above spontaneous level). Each adaptation experiment was followed by a period of recovery, during which we verified that responses had returned to pre-adaptation levels. The ratios of the post-adaptation and pre-adaptation contrasts needed to elicit the criterion response are shown on each of the four lower graphs; values greater than 1 indicate an elevation of this 'threshold', values less than 1 a reduction.

monocularly driven neurone with unusually broad orientation selectivity, showed no reliable effects. Spontaneous activity was not always depressed by adaptation.

We were especially interested to know whether the reduction in sensitivity that followed adaptation was confined to stimuli of the same spatial frequency as the adapting pattern; we examined this question in experiments where the spatial frequencies of the test and adapting patterns were varied. The graphs in Fig. 3a show contrast-response relationships for a complex cell, obtained with gratings of 0.5 and 1.5 c deg^{-1} ; vertical bars mark the contrast needed to produce a response of $10 \text{ impulses s}^{-1}$. The graphs in Fig. 3b show the same relationships obtained during adaptation to a grating of 0.5 c deg^{-1} . Responses to gratings of this spatial frequency were much reduced by adaptation (the contrast required to produce a response of $10 \text{ impulses s}^{-1}$ was elevated by a factor of more than three), whereas responses to gratings of 1.5 c deg^{-1} were hardly affected. The graphs in Fig. 3c show the results of a complementary experiment, in which the spatial frequency of the adapting grating was 1.5 c deg^{-1} . In this case, sensitivity for test gratings of 0.5 c deg^{-1} was unaffected, whereas sensitivity for gratings of 1.5 c deg^{-1} was much reduced. The effect of adaptation on this neurone cannot be explained by a uniform change in sensitivity; some spatial frequency selective mechanism must be involved. We observed behaviour of this kind in all seven complex neurones in which we studied the spatial frequency selectivity of adaptation; of three simple cells, one seemed to show spatial frequency selective adaptation, one definitely did not, and the third gave variable results.

Our experiments confirm two earlier reports that the behaviour of cortical neurones may be altered following a period of prolonged stimulation^{9,10}. However, it is necessary to

measure sensitivity in order to characterise the changes adequately; alterations in spontaneous activity, or in responses to high-contrast stimuli, provide an incomplete description of the effects of adaptation.

Our most surprising observation is that the loss of sensitivity in cortical neurones can be specific to the adapting stimulus. Our observations on complex cells might be explained by supposing that each complex cell is excited by a group of simple cells having different frequency selectivities. However, because the spatial frequency selectivity of complex cells is little different from that of simple cells¹¹, and because we have observed frequency-selective adaptation in a simple cell, this is not a convincing argument. Alternatively, adaptation in both simple and complex cells might result from changes in the afferent relay from the lateral geniculate nucleus. Again, this seems unlikely, as the spatial frequency selectivity of cortical neurones is far greater than that of geniculate neurones¹², and also as we have found in separate experiments that the responses of geniculate cells are unaffected by adaptation. We are left with the suggestion that some pattern-selective, perhaps inhibitory, cortical mechanism is involved.

Our observed frequency-selective loss of sensitivity is inconsistent with some models of perceptual after-effects^{2,5}. Although our experiments do not question the idea that human perceptual adaptation reflects adaptation in cortical neurones, they do limit the inferences about the properties of these neurones that may be based on adaptation experiments in man.

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Effects of calcium on the intensity-response curve of toad rods

THERE is evidence that Ca^{2+} is stored in the saccules in the outer segments of vertebrate rods, and released by light (for review, see ref. 1). Thus, light apparently increases $[\text{Ca}^{2+}]_i$, the cytoplasmic Ca^{2+} concentration. It now seems unlikely that this increase of $[\text{Ca}^{2+}]_i$ serves as an internal transmitter in generating the light response²⁻⁴. However, there is compelling evidence in the ventral photoreceptors of *Limulus*^{5,6} that it reduces photoreceptor sensitivity during light adaptation. In vertebrate rods an alteration of $[\text{Ca}^{2+}]_o$, the external Ca^{2+} concentration, alters $[\text{Ca}^{2+}]_i$, and the primary effects of changing $[\text{Ca}^{2+}]_o$ seem to be exerted inside the cell^{1,7,8}. Increased $[\text{Ca}^{2+}]_o$ mimics certain aspects of light adaptation, including hyperpolarisation of the resting membrane potential and reduction of light responses⁹⁻¹¹. Light adaptation also shifts the entire

intensity-response function (the V -log I curve) towards higher intensities on the intensity axis, thus reducing the sensitivity of toad rods¹¹. We report here that increased $[\text{Ca}^{2+}]_o$ can mimic this important aspect of light adaptation, whereas lowered $[\text{Ca}^{2+}]_o$ mimics dark adaptation. Control experiments indicate why altered $[\text{Ca}^{2+}]_o$ did not shift the V -log I curve in the recent work of Lipton, Ostroy and Dowling¹¹, who thus concluded that Ca^{2+} does not control this major aspect of photoreceptor sensitivity.

Experiments were carried out in the isolated, inverted and superfused retina of the toad, *Bufo marinus*, as described previously¹⁰. The receptor surface was viewed with a high-resolution infrared imaging system, and only retinal areas were used where the receptors were normal in appearance over a large area. Contact of the electrode tip with the distal end of an outer segment was readily visualised, and most recordings were from outer segments¹⁰. Deeper recordings, some of which were probably from inner segments, gave results similar to those obtained in outer segments. All recordings were from red rods, as determined by maximum sensitivity at about 500 nm. Ultra-fine micropipette electrodes were formed and advanced by previously described techniques¹²; intracellular recordings were readily obtained showing relatively large membrane potentials and light responses that could be maintained stably for up to 3 h. Light stimuli were introduced from below and focused by the condenser system of the microscope. Our control perfusate contained: NaCl (94 mM), KCl (2 mM), MgCl_2 (1 mM), NaHCO_3 (15 mM), glucose (10 mM) and CaCl_2 (1.8 mM). After bubbling with 98.5% O_2 and 1.5% CO_2 , it had a pH of 7.7-7.8; the temperature was maintained at 21-23°C. In this solution both the concentration of bicarbonate and the pH at our maintained temperature were based on an analysis of blood plasma in *Bufo marinus*¹³.

Figure 1 shows fully dark adapted V -log I curves of a single cell at three values of $[\text{Ca}^{2+}]_o$. It is critical to test ionic effects on photoreceptor sensitivity in dark adapted conditions, as ionic alteration of sensitivity to a steady background tends to cancel the measured alteration of sensitivity to the test stimulus. The reasons for this, and our methods for ensuring complete dark adaptation, have been described elsewhere¹⁴. Each V -log I curve required about 15 min, because of the stimulus spacing necessary to maintain dark adaptation. Following each change of solution, turnover in the perfusion chamber required less than 1 min. At least 5 min were always allowed before taking records, at which time the resting membrane potential and light responses had stabilised in the new solution. In Fig. 1 the value of $[\text{Ca}^{2+}]_o$ was at the control level of 1.8 mM for curve 1, then elevated to 6.0 mM for curve 2, returned to 1.8 mM for curve 3, lowered to 0.3 mM for curve 4, and again returned to 1.8 mM for curve 5. Each smooth curve in Fig. 1 is the computer-generated best fit to the data points, using the method of least squares and the Naka-Rushton equation, $V/V_{\max} = I^n / (I^n + \sigma^n)$. In this equation, V is the peak response amplitude elicited by a given stimulus intensity, I , and $V_{\max}$ is the maximum (saturated) response amplitude. The constant, σ , is the stimulus intensity eliciting a response of one-half the maximum amplitude, and in normalised curves the constant, n , is proportional to the slope at σ .

As previously demonstrated¹¹, and confirmed by us, the effects of light adaptation on the V -log I curve of toad rods include a reduction of $V_{\max}$, a reduced slope of the rapidly rising central portion of the curve, and a shift of σ to the right (towards higher intensities). Figure 1 shows that all these effects were mimicked by increasing $[\text{Ca}^{2+}]_o$ from 1.8 to 6.0 mM, whereas lowering $[\text{Ca}^{2+}]_o$ from 1.8 to 0.3 mM had the opposite effects. After the supra-normal $[\text{Ca}^{2+}]_o$, curve 3 shows almost complete recovery on return to the control solution. Following the sub-normal $[\text{Ca}^{2+}]_o$, curve 5 shows a more distinct loss of response amplitude on return to the control solution. In another experiment, however, when we waited 30 min after returning from 0.3 mM to 1.8 mM of $[\text{Ca}^{2+}]_o$, the final curve closely matched the initial control curve. As the effects of elevated and lowered

$[Ca^{2+}]_o$ were reversible, they apparently did not damage the photoreceptors. Effects of both elevated and lowered $[Ca^{2+}]_o$ have been tested in 10 different cells, and the results have been highly consistent. When $[Ca^{2+}]_o$ was increased from 1.8 to 6.0 mM the σ shift averaged +0.26 log units, and when $[Ca^{2+}]_o$ was lowered from 1.8 to 0.3 mM the σ shift averaged -0.49 log units. In both cases the s.e.m. was 0.04 log units. These σ shifts seem highly significant, as they show that altered $[Ca^{2+}]_o$ can mimic this important aspect of adaptation, with σ serving as a conventional index of the position of the V -log I curve on the intensity axis.

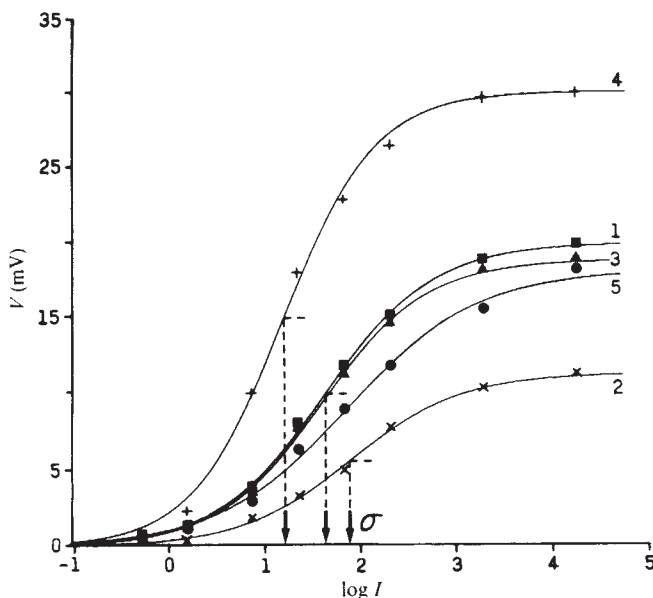


Fig. 1 Effects of $[Ca^{2+}]_o$ on the V -log I curve recorded intracellularly in the outer segment of a dark adapted red rod. Each point represents peak response amplitude. For each curve, responses were recorded in the order of increasing intensity. The sequence of curves is indicated by the numbers at the right. $[Ca^{2+}]_o$ was 0.3 mM for curve 4, 1.8 mM for curves 1, 3 and 5, and 6.0 mM for curve 2. A circular stimulus spot was centred on the electrode tip and focused on the outer segments, using direct viewing with infrared light in the stimulus beam. The stimulus spot was 1.0 mm in diameter at the outer segments, which more than covered the rod's receptive field¹⁷. Flashes of 500 nm light were 100 ms in duration, with a sufficient interval after each flash to maintain dark adaptation¹⁴. Stimulus intensities are expressed in photons absorbed per rod-flash, as described elsewhere¹⁴. The recording bandpass was 0–30 Hz, which did not affect response time course or amplitude. The resting membrane potential in control solution was -38 mV and showed no significant change during the experiment. In elevated $[Ca^{2+}]_o$ the membrane hyperpolarised by about 1.0 mV, and in lowered $[Ca^{2+}]_o$ depolarised by about 6.0 mV. With raised $[Ca^{2+}]_o$ the value of n remained constant at the control value of 0.75, so in high $[Ca^{2+}]_o$ the reduced slope of the V -log I curve resulted only from the decrease of V_{max} . In lowered $[Ca^{2+}]_o$ the value of n increased to 0.86, which contributed part of the observed increase in slope of the V -log I curve.

In their work on toad rods, Lipton *et al.* observed effects of $[Ca^{2+}]_o$ on V_{max} and the slope of the V -log I curve that were similar to the effects reported here. We therefore examined why they did not observe effects of altered $[Ca^{2+}]_o$ on the value of σ , even when the preparation was reported to be dark adapted. First, our perfusate contained 15 mM of bicarbonate, which was used for buffering. The perfusate of Lipton *et al.* contained only 0.13 mM of bicarbonate, and buffering was done with HEPES.

Our higher bicarbonate concentration seems preferable, as it is based on a plasma analysis in *Bufo marinus*¹³. Also, there is evidence that a bicarbonate concentration of about 25 mM is required in some other vertebrates for maintaining normal function of either photoreceptors¹⁵ or the adjacent pigment epithelium¹⁶. When both solutions were tested on the same cells, our control perfusate yielded V -log I curves with significantly higher values of V_{max} than obtained in the control perfusate of Lipton *et al.* When $[Ca^{2+}]_o$ was raised to 6.0 mM or lowered to 0.3 mM, we found σ shifts in their perfusate, but these were smaller and less consistent than those obtained in our own perfusate. Second, Lipton *et al.* increased $[Ca^{2+}]_o$ to only 3.2 mM, which we found to have little effect on σ , whereas our higher value of 6.0 mM was significantly more effective. Third, Lipton *et al.* used 1.0 mM of EGTA to produce a very low $[Ca^{2+}]_o$ of about 10^{-9} M. As demonstrated by them, and confirmed by us, this results in a continuous loss of sensitivity until the cell is almost completely unresponsive. As this occurred, they showed that the value of σ in the V -log I curves moved rapidly to the right. Their earliest curve was taken between 4 and 6 min after starting superfusion with the low $[Ca^{2+}]_o$; this was the curve chosen for comparison with the control solution and which showed no σ shift from control conditions. By extrapolation, curves taken at earlier times would be expected to show a σ shift to the left, but such curves were not shown. It is not clear why a given curve was chosen, from a rapidly changing set of curves, for comparison with the curve taken in normal $[Ca^{2+}]_o$. Fourth, our results indicate that a V -log I curve cannot be obtained within 2 min without significantly light adapting the cell by the test stimuli. Hence, if the low $[Ca^{2+}]_o$ of Lipton *et al.* shifted σ to the left, this may have been masked by a light adaptive shift of σ to the right. Because of these problems in very low $[Ca^{2+}]_o$, our approach was to use a subnormal value of $[Ca^{2+}]_o$ that was sufficiently low to yield clear effects but not low enough to cause continuous loss of sensitivity. The value of 0.3 mM proved adequate for this purpose, as it provided a steady-state condition during which a fully dark adapted V -log I curve could be obtained.

Thus, our results indicate that alteration of $[Ca^{2+}]_o$ can control the value of σ in the V -log I curve of toad rods. These results are consistent with a role for $[Ca^{2+}]_i$ in the control of sensitivity in vertebrate photoreceptors. Hence, changes of $[Ca^{2+}]_i$ may partially control the changes of sensitivity that occur during light and dark adaptation.

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Irazepine, a noncompetitive, irreversible inhibitor of ^3H -diazepam binding to benzodiazepine receptors

THE identification and characterisation of stereospecific, high-affinity binding sites for benzodiazepines in the mammalian CNS has been described both *in vivo*¹⁻³ and *in vitro*⁴⁻⁶. The correlations obtained between the *in vitro* affinities of a series of benzodiazepines for these binding sites and their clinical potencies as muscle relaxants², anxiolytics⁶ and anticonvulsants⁶ strongly suggest that these sites are pharmacological receptors mediating the therapeutic actions of the benzodiazepines. Benzodiazepines such as clonazepam, oxazepam and flurazepam are rapidly reversible, competitive inhibitors of ^3H -diazepam binding to synaptosomal membranes^{7,8}. Competitive inhibition of ^3H -diazepam binding has also been observed with the triazolopyridazines, which possess both anticonvulsant and anxiolytic activity⁹, and with the purines inosine and hypoxanthine^{10,11}, which may be endogenous ligands to the benzodiazepine receptor¹⁰⁻¹⁴. The development of noncompetitive, irreversible ligands has been invaluable in the characterisation, isolation and purification of both peripheral and central neurotransmitter receptors¹⁵, and may also prove useful for studying the benzodiazepine receptor. We now report the synthesis of a novel benzodiazepine, irazepine, which acts as a noncompetitive, irreversible inhibitor of ^3H -diazepam binding to synaptosomal membranes.

Irazepine [1-(2-isothiocyanatoethyl)-7-chloro-1,3-dihydro-5-(2-fluorophenyl)-2H-1,4-benzodiazepine-2-one] (Fig. 1a) was prepared as follows. A mixture of 203 mg (0.5 mmol) of Ro7-1986/1 (Fig. 1b), 10 ml of H_2O , 25 ml of CHCl_3 and 252 mg (3 mmol) of NaHCO_3 was stirred vigorously for 10 min, and then treated with 80.5 mg (0.7 mmol) of redistilled thiophosgene. Following vigorous stirring for 60 min, the CHCl_3 layer was separated, washed with 10 ml H_2O , dried with MgSO_4 and evaporated to give 182 mg of irazepine as a yellow foam. Thin layer chromatography of this material (silica gel GF, ether, ultraviolet visualisation) revealed essentially only one spot. The yellow foam was dissolved in 3 ml of warm isopropanol, rendered acidic with a slight excess of HCl gas and cooled to give 164 mg (80% yield) of crystalline irazepine·HCl. This material was recrystallised from methanol-diethyl ether to give analytically pure material: melting point (m.p.), 217–220 °C; mass spectrum, m/e 373/375 (M^+), infrared ν_{max} 2,110 cm^{-1} (NCS). Analysis: calculated for $\text{C}_{18}\text{H}_{14}\text{Cl}_2\text{FN}_3\text{OS}$: C, 52.69; H, 3.44; N, 10.24. Found: C, 52.42; H, 3.65; N, 10.14. In another preparation, the foamy residue from evaporation of the CHCl_3 extract was crystallised from isopropanol to give 151 mg (80% yield) of irazepine as the crystalline base, m.p. 142–144 °C. This material was readily recrystallised from absolute ethanol to give pure irazepine, m.p. 144.5–146 °C.

Specific binding of ^3H -diazepam to crude synaptosomal fractions of rat cerebral cortex was determined as described previously¹⁶, with the following modifications. Synaptosomal

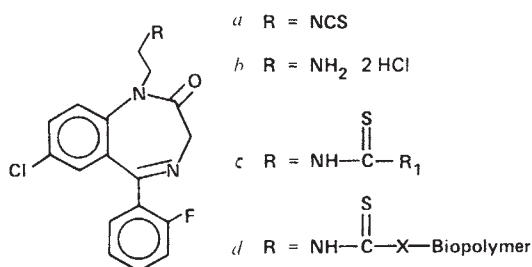


Fig. 1 Structure of irazepine (a), Ro7-1986/1 (b) and presumed reaction products (c, d) with bionucleophiles.

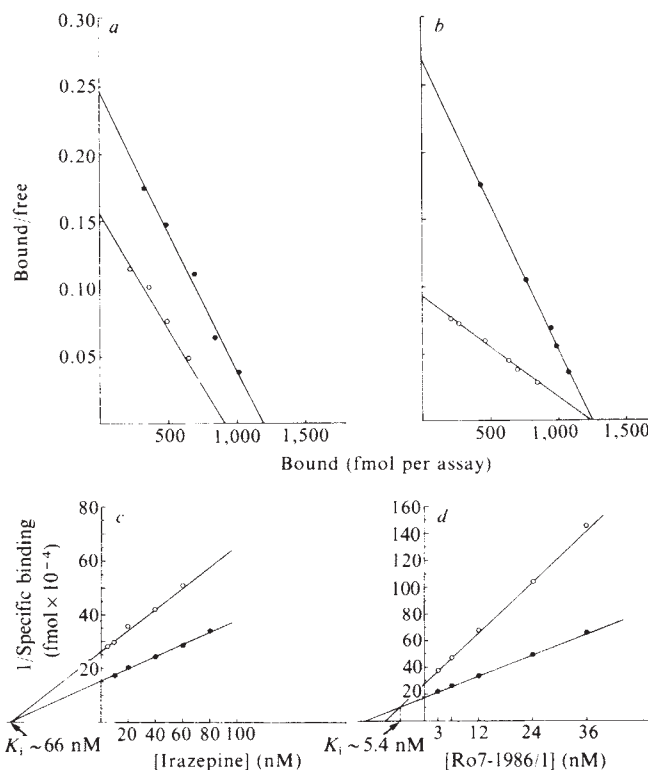


Fig. 2 Scatchard analysis of ^3H -diazepam binding to rat synaptosomal membranes in the presence of: a, irazepine·HCl (20 nM) (○), control (●); b, Ro7-1986/1 (6 nM) (○), control (●). Dixon plot of ^3H -diazepam binding in the presence of variable concentrations of c, irazepine·HCl. 2.88 nM (○) and 6.77 nM (●) ^3H -diazepam and d, Ro7-1986/1: 3.33 nM (○) and 7.03 nM (●) ^3H -diazepam. Plots are representative experiments; individual data points were determined in triplicate for total binding and duplicate for nonspecific binding (see ref. 16).

membranes were incubated with irazepine·HCl, Ro7-1986/1, clonazepam or distilled water at 0–4 °C for 30 min before addition of ^3H -diazepam. The incubation was then continued for 20 min at 0–4 °C, and terminated by filtration as described previously. Clonazepam (final concentration 1 μM) was used to determine nonspecific binding.

The inhibition of ^3H -diazepam binding by Ro7-1986/1 and irazepine was characterised *in vitro* by Scatchard and Dixon analyses (Fig. 2). No significant change in the maximum number of binding sites (B_{max}) was observed when synaptosomal membranes were incubated with Ro7-1986/1, whereas there was a concentration-dependent decrease in the apparent affinity using between 6 and 18 nM Ro7-1986/1 (Fig. 2b and unpublished observations). In contrast, incubation of tissue with irazepine·HCl (20 nM) resulted in a significant reduction in B_{max} ($21.4 \pm 3.8\%$ reduction in five experiments; $P = 0.02$) and a consistent but not statistically significant increase in K_d (3.7 ± 0.3 nM compared with 4.6 ± 0.5 nM; $P = 0.1$) (Fig. 2a). These findings are consistent with a competitive inhibition of ^3H -diazepam binding by Ro7-1986/1 and a noncompetitive inhibition by irazepine·HCl. These observations were confirmed by Dixon analyses of the inhibition of ^3H -diazepam binding by the two compounds (Fig. 2c,d). The K_i values for Ro7-1986/1 and irazepine·HCl were estimated to be 5.4 and 66 nM, respectively.

The noncompetitive inhibition of ^3H -diazepam binding by irazepine indicated that the binding of this ligand, in contrast to that of other benzodiazepines, may be irreversible. Thus, the binding of ^3H -diazepam was then studied in the presence of a

fixed concentration of irazepine·HCl and a variable amount of receptor (tissue) (see ref. 17) in a manner analogous to that used for determination of enzyme inhibition by noncompetitive inhibitors. The effects of increasing receptor (tissue) concentrations on the inhibition of ^3H -diazepam binding by irazepine·HCl suggest an irreversible inhibitor (Fig. 3). These observations are further supported by the ready reaction of irazepine with certain model bionucleophiles.

To determine the reactivity of irazepine with model bionucleophiles ($-\text{NH}$, $-\text{SH}$ and $-\text{OH}$ groups) *in vitro*, irazepine base was treated in 70% aqueous ethanol (at 25 °C) with an excess of compounds containing these functional groups. The relative velocity of the reactions was monitored by observing the disappearance of irazepine by thin-layer chromatography. Irazepine reacted rapidly with both diethylamine and methyl mercaptoacetate. These reactions were virtually complete in less than 5 min, presumably yielding (Fig. 1) the corresponding thiourea (Fig. 1c, $\text{R}_1 = \text{N}(\text{C}_2\text{H}_5)_2$) and the dithiocarbamate (Fig. 1c, $\text{R}_1 = \text{S}-\text{CH}_2\text{CO}_2\text{CH}_3$) derivatives, respectively. In contrast, reduced glutathione reacted slowly with irazepine in these conditions; a substantial amount of irazepine remained after 24 h. Little, if any, reaction of irazepine with 70% ethanol was observed after 24 h. The reaction of the isothiocyanate function with primary and secondary amines as well as with mercaptans has been described (for review, see ref. 18), and with aliphatic amines the reaction is usually quite rapid¹⁹. These observations suggest that a covalent binding of irazepine to the benzodiazepine receptor could occur by reaction with a free amino or sulphhydryl group, for example, from a lysine or cysteine residue, yielding a thiourea or dithiocarbamate (as in Fig. 2d, $\text{X} = \text{NH}$ or S , respectively). These *in vitro* studies do not, however, exclude a covalent link between irazepine and other bionucleophiles possibly associated with the receptor (such as a hydroxy or guanidino moiety).

Our data suggest that in the conditions used, irazepine is a noncompetitive, irreversible inhibitor of ^3H -diazepam binding. This activity is unique, as the binding of other benzodiazepines,

triazolopyridazines and purines to the benzodiazepine receptor⁷⁻¹¹ is competitive. Irazepine is potentially useful in receptor isolation and purification, biochemical characterisation and histochemical localisation of the receptor, as well as estimation of receptor turnover *in vivo*. Further studies will be necessary to test the usefulness of this compound as a probe of the benzodiazepine receptor.

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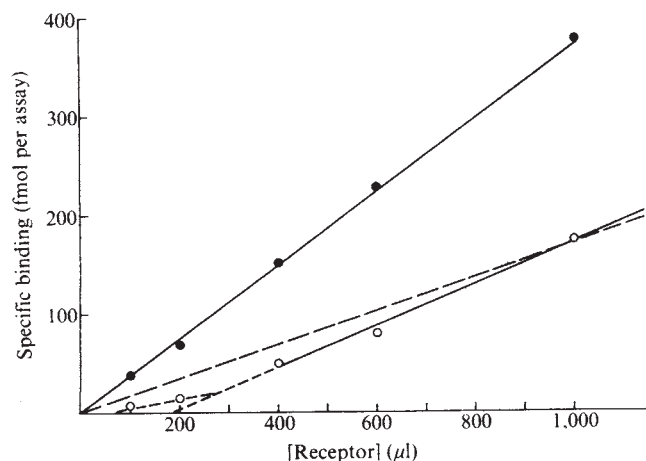


Fig. 3 Determination of irreversible nature of irazepine binding to benzodiazepine receptors *in vitro*. Ordinate: specific binding (fmol per mg protein); abscissa: μl of receptor (tissue) added. Incubations were performed as described in text. Upper line: observed points for control incubations (no irazepine); middle line: theoretical line for a noncompetitive reversible inhibitor, assuming the lines should pass through the origin and the 1,000 μl per incubation point. Bottom line: observed points in the presence of 100 nM irazepine·HCl. The line generated in the presence of irazepine indicates irreversible binding, analogous to determination of reversible/irreversible nature of enzyme inhibitors (see ref. 17) at low concentrations that is, of receptor, virtually no binding of ^3H -diazepam is observed. ○, Irazepine; ●, control; ---, reversible (expected).

Collagenous bone matrix is a local mitogen

THE importance of collagens in growth and differentiation^{1,2} and in cell adhesion³ is well documented. We have previously shown that cell-free, demineralised, collagenous bone matrix induces the formation of cartilage, bone and bone marrow⁴⁻⁶. One of the early responses to implanted collagenous matrix is the appearance of connective tissue mesenchymal cells adjacent to the implanted matrix before their differentiation into cartilage. We present here direct evidence that collagenous bone matrix is a local mitogen for connective tissue cells. We find that the collagenous bone matrix is a potent inducer of ornithine decarboxylase (EC 4.1.1.17, ODC), an enzyme involved in polyamine biosynthesis⁷ and considered as an early marker of cell proliferation⁸. These studies have been corroborated by incorporation of ^3H -thymidine into acid-precipitable material and by autoradiography. Our results reveal the local influence of insoluble collagenous bone matrix on proliferation of mesenchymal cells and imply a role for this extracellular matrix in anchorage-dependent⁹ events in cell growth and differentiation.

Demineralised bone matrix powder prepared from rat diaphyses was transplanted subcutaneously in the anterior thoracic region of 28-35-d-old, male rats of Long-Evans strain

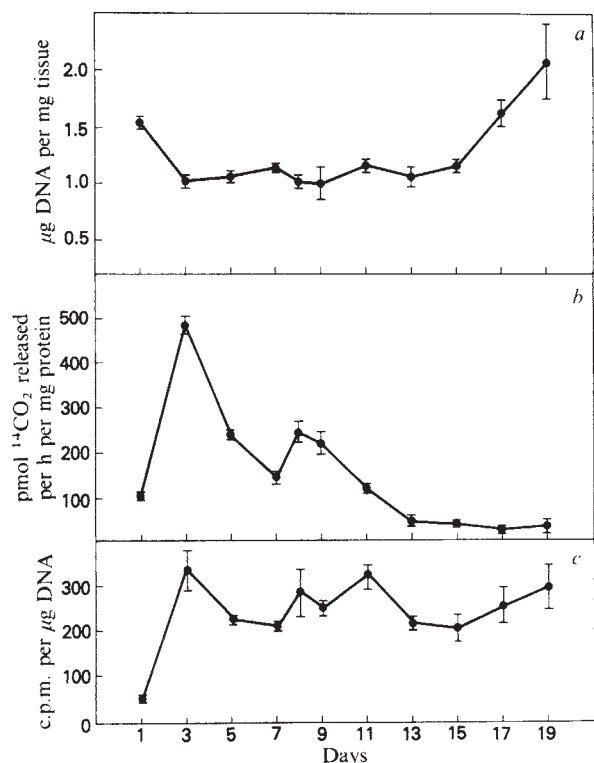


Fig. 1 Changes in the concentration of DNA (a), ornithine decarboxylase (b) and ^3H -thymidine incorporation (c) during collagenous matrix-induced endochondral bone development. Plaques from individual rats were homogenised in ice-cold buffer containing 50 mM Tris-HCl, 10 mM dithiothreitol and 1 mM EDTA at pH 7.4, and centrifuged for 30 min at 30,000 g. ODC in the supernatant was determined by the method of Jänne and Williams-Ashman¹⁰; protein was determined according to Lowry *et al.*¹³. ^3H -thymidine incorporation and DNA content were determined as described in the text. Day 1, polymorphonuclear leukocytes; day 3, fibroblasts; day 7, cartilage; day 11, bone; day 19, bone marrow.

as described previously⁴. The day of transplantation was designated as day 0. On different days after transplantation, the rats were injected intravenously with ^3H -thymidine at a dose of 1 μCi per g body weight, 2 h before the end of the experiment. At autopsy the plaques were dissected free of adhering tissue; one portion from each plaque was used for determination of the activity of ODC^{10,11}, and another portion was processed for autoradiography by conventional methods with Kodak NTB-3 nuclear track emulsion. The rest of the plaque was homogenised in cold 10% trichloroacetic acid (TCA) containing 1 mM thymidine, and the incorporation of ^3H -thymidine into acid soluble and insoluble portions was determined. Following acid hydrolysis, DNA in the TCA extract was measured. Calf thymus DNA served as the standard¹². The results were statistically evaluated by means of the Student's *t* test.

On day 3 collagenous bone matrix induced an increase in the ODC activity (Fig. 1b), which correlated with the incorporation of ^3H -thymidine into the acid-precipitable fraction (Fig. 1c). Autoradiography at the light microscope level revealed cells labelled with ^3H -thymidine in the vicinity of the implanted matrix (Fig. 2a). The high content of DNA on day 1, as determined by the diphenylamine method¹², is attributed to migration of polymorphonuclear leukocytes into the implant site, because there was no detectable incorporation of ^3H -thymidine. There was a decline in both ODC activity and ^3H -thymidine incorporation on days 5–7, when cartilage differentiation was in progress. On days 8–9 there was a second

peak of proliferation of cells (Fig. 2b) before impending osteogenesis and vascular invasion, as indicated by a peak of ODC and ^3H -thymidine incorporation (Fig. 1). The ODC activity then steadily declined, but incorporation of ^3H -thymidine continued and increased further during haematopoiesis on days 18–21. This increased incorporation was correlated with an increase in accumulated DNA as determined by a chemical procedure¹².

The proliferative response to collagenous bone matrix seems to be quite specific, because collagenous matrix from rat tail tendon is weak in this respect¹¹. It is worth recalling that tendon matrix is incapable of inducing endochondral bone¹.

The mechanism(s) underlying the local mitogenic action of the collagenous bone matrix is not known. Although the role of soluble growth factors in cell proliferation has been studied¹⁴, virtually nothing is known about the role of insoluble, extracellular, matrix macromolecules such as collagen and associated components. In many tissues collagenous matrix is the extracellular framework with which cells interact. The importance of collagenous matrix in development and differentiation and in the specification of positional information has been emphasised earlier¹. Recent investigations show that collagen is required for the proliferation of normal connective tissue cells but not for their transformed counterparts¹⁶. The study of skeletal and dental tissues, which abound with these constituents, may provide important clues to the possible function of the extracellular collagenous matrix and its associated components. The cellular

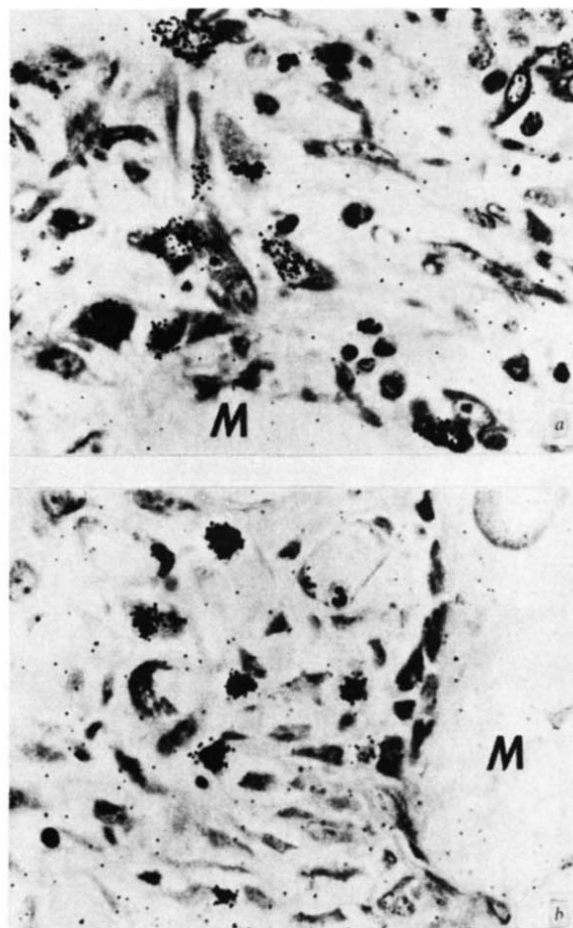


Fig. 2 Autoradiographic localisation of ^3H -thymidine incorporation in the collagenous matrix-induced plaques on different days following implantation of matrix. a, Day 3, the label is mainly in the mesenchymal cells in the vicinity of the matrix particles (M). $\times 860$. b, Day 8, label is localised in the mesenchymal cells $\times 860$. M, matrix particle.

cascade accompanying the collagenous bone matrix-induced formation of endochondral bone^{1,4-6} is remarkably similar to that seen during fracture healing in man. Collagens and their peptide constituents are chemotactic to fibroblasts¹⁵. The present study provides evidence that collagenous matrix from bone has a local mitogenic action. These observations may explain the local nature of fracture and wound healing responses and also provide a basis for further studies in clinical areas such as dental implants and periodontal diseases.

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Mutagenesis *in vitro* by DNA polymerase from an RNA tumour virus

THE fidelity of DNA synthesis has usually been determined from polynucleotide templates of restricted base composition¹⁻³. Mis-matched nucleotides were simply identified as those non-complementary to the template. Error rates estimated in this way are less than were predicted on the basis of Watson-Crick base pairing, 10^{-2} (refs 4-6), but greater than were inferred from spontaneous mutation frequencies, 10^{-8} - 10^{-11} (ref. 7). However, *in vitro* error rates measured for the same enzyme vary from one synthetic template to another^{3,8,9}. In addition, the repeating structure of homopolymers and copolymers may, by slippage and looping-out of mis-matched bases¹⁰, allow a higher frequency of mis-pairing than natural DNA of random sequence. To avoid these problems, we have recently developed a method for measuring the accuracy of *in vitro* DNA synthesis using a natural DNA template¹¹. We now report that a DNA polymerase which has a high frequency of mis-incorporation with synthetic polynucleotides¹ is also mutagenic when copying natural DNA *in vitro*.

Briefly, mutagenesis is measured *in vitro* with biologically active DNA in the following way. The template is the single-stranded circular DNA from bacteriophage Φ X174 and contains one nucleotide substitution mutation (*am3*) located in the overlapping genes *D* and *E* (refs 12, 13). Synthesis is initiated on a DNA restriction fragment (*Hae*III Z-5) hybridised to the template. The 3'-OH terminus of Z-5 is located 83 nucleotides from the *amber* mutation¹³. Purified DNA polymerase from any source may be used to copy the DNA in defined *in vitro* conditions. The accuracy with which the DNA polymerase has copied the *amber* mutation is then measured by

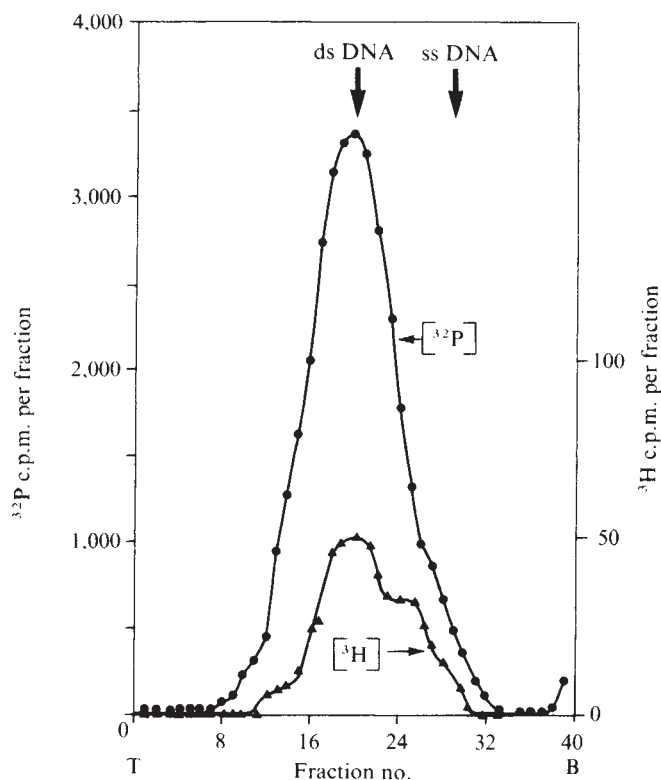


Fig. 1 Sedimentation of Φ X174 DNA after copying by AMV DNA polymerase. 3 H-labelled *am3* Φ X174 DNA was hybridised to the Z-5 restriction fragment and copied by AMV DNA polymerase as indicated in the legend to Table 1. A 20- μ l sample of the reaction mixture after termination with EDTA (before top annealing) was applied to a 5-20% neutral sucrose gradient containing 10 mM Tris-HCl (pH 7.5), 10 mM EDTA, 1.0 M NaCl and 0.1% sarkosyl. Gradients were centrifuged at 45,000 r.p.m. at 20°C for 16 h in a Spinco SW 50 rotor. Fractionation of the gradients was from the top, and the total amount of radioactivity present in each fraction was determined after adding 50 μ l calf thymus DNA (1 mg ml⁻¹), 1 ml 5% trichloroacetic acid and collecting the precipitates on glass fibre filters. 3 H-radioactivity was determined after the 32 P-radioactivity had decayed for 4 weeks. The ss DNA marker was single-stranded 3 H- Φ X DNA. The ds DNA marker was 14 C-labelled double-stranded Φ X DNA. In control experiments, the single-stranded Φ X174 DNA hybridised to the Z-5 fragment sedimented coincidentally with the single-stranded Φ X174 DNA marker.

transfection. The copied DNA is used to infect *Escherichia coli* spheroplasts. Accurately copied DNA will contain the *amber* codon in both parent and daughter strands, and give rise to mutant phage only. Inaccurately copied DNA will contain mis-matched bases, some of which will code for a revertant daughter strand at the *am3* codon. The fidelity of DNA synthesis *in vitro* is obtained from the reversion frequency of the phage produced from copied DNA.

Φ X *am3* DNA was copied by homogeneous avian myeloblastosis virus (AMV) DNA polymerase in the presence of 8 mM MgCl₂ and 100 μ M of each of the deoxynucleoside triphosphates and then used to infect *E. coli* spheroplasts. The infected spheroplasts were immediately plated onto indicator bacteria either permissive or non-permissive for *am3*, to determine how many infective centres produced mutant *am3* phage and how many revertant phage (Table 1). *am3* DNA copied by AMV DNA polymerase produced phage with an average reversion frequency of 25.8×10^{-5} . This is about 4.5 times higher than the average background reversion frequency, 5.76×10^{-5} , which results from infection by the same *am3* DNA template incubated with reaction mixture in the absence of DNA polymerase. These results indicate that copying by AMV

Table 1 Reversion of Φ X174 *am3* DNA after copying by AMV DNA polymerase

Expt	Nucleotides incorporated per Φ X template	Total infective centres	Wild-type infective centres	Reversion frequency	Calculated error rate
1	0	115,200	5	4.34×10^{-5}	1/392
	1,410	122,800	46	3.75×10^{-4}	
2	0	819,000	37	4.52×10^{-5}	1/937
	1,080	130,700	24	1.84×10^{-4}	
3	0	30,000	1	3.33×10^{-5}	1/1,300
	520	15,000	2	1.33×10^{-4}	
4	0	830,000	20	2.41×10^{-5}	1/820
	497	10,900	2	1.83×10^{-4}	
5	0	3,890,000	455	1.17×10^{-4}	1/1,645
	420	428,000	84	1.96×10^{-4}	
6	0	11,525,000	950	8.24×10^{-5}	1/329
	767	460,000	220	4.78×10^{-4}	

am3 Φ X DNA was copied *in vitro* at 37 °C for 45 min in a 50- μ l reaction mixture containing 40 mM Tris-HCl (pH 7.5), 5 mM dithiothreitol, 8 mM MgCl₂, 2×10^{11} molecules of Φ X174 *am3* single-stranded DNA hybridised to an *am3* *Hae*III Z-5 fragment, 100 μ M each of dATP, dGTP, dCTP and [α -³²P]dTTP (specific activity, 200 d.p.m. pmol⁻¹), and 10 pmol of purified AMV DNA polymerase. Synthesis was terminated by the addition of EDTA to 20 mM, and acid-precipitable counts were determined on aliquots of 2 μ l. The number of nucleotides incorporated per template was determined by dividing the number of nucleotides incorporated by the number of Φ X template molecules added to the reaction mixture (5,376 nucleotides per genome). The remainder of the copied DNA was hybridised to *Hae*III restriction fragments from *am3* Φ X DNA ('top annealing', ref. 11) and used to infect spheroplasts from *E. coli* KT-1 (*su*⁺, λ ⁻, Φ X⁺) as described by L.A.W. and L.A.L.¹¹ In experiments 5 and 6, top annealing was unnecessary¹⁹. The infected spheroplasts were immediately plated onto indicator bacteria *E. coli* HF 4704 and *E. coli* 4714 (restrictive and permissive for the *amber* codon) to assay the number of wild-type and total infective centres. Uncopied DNA was subjected to exactly the same conditions except that DNA polymerase was omitted from the reaction mixture. The AMV DNA polymerase used in this study was purified to homogeneity from isolated virions by the method of Kacian *et al.*²⁰. The preparation used was free of detectable DNA, endonuclease, exodeoxyribonuclease and DNA ligase²¹.

DNA polymerase has led to an increased frequency of back mutations (revertants) on the Φ X DNA.

To calculate the frequency of revertants in the daughter DNA strand from the reversion frequency of the phage after infection, it is necessary to correct for penetrance, the frequency with which the *in vitro* synthesised DNA strand is expressed in the progeny phage. A value of 0.13 ± 0.06 was obtained from six experiments in which spheroplasts were infected with *am3* Φ X (+)strand DNA circles hybridised to a saturating concentration of wild-type restriction fragments (*Hae*III Z-7) which cover the *am3* codon¹¹. The calculated error rates of DNA synthesis past the *am3* mutation by AMV DNA polymerase therefore averaged 1 in 904 in the six determinations (Table 1).

Our calculations assume that every infecting Φ X DNA molecule contained a daughter DNA strand which extended at least past the *am3* codon. This assumption was tested by sedimentation analysis of the Φ X DNA product following *in vitro* synthesis by AMV DNA polymerase. The single-stranded Φ X DNA template was labelled with tritium and synthesis was monitored by incorporation of [α -³²P]dTTP. The DNA product, copied to the extent of an average of 1,050 nucleotides per added template, was sedimented in a neutral, high salt sucrose gradient. In these conditions, Φ X DNA circles migrate as double-stranded molecules when they are replicated 20% or more¹⁴. After synthesis, 70% or more of the templates and all of the product sediment coincidentally with double-stranded Φ X174 DNA (Fig. 1). (The Z-5 primer plus the 83 nucleotides to the end of the *am3* codon constitute 393 nucleotides, or only 7.3% of the 5,376 nucleotide-long genome¹³.) To the degree that some molecules of template remain uncopied, the error rate could be underestimated by as much as 30%.

These experiments represent the first estimate of the error rate with which AMV DNA polymerase copies a natural DNA template *in vitro*. The error rates are similar to those observed with homopolymer templates. This measurement with Φ X DNA does not take into account substitutions at the *amber* codon that are lethal, or the effects of substitutions at other positions on the genome. Studies with *E. coli* DNA polymerase

in mutagenic conditions indicate that distant incorrect substitutions invariably decrease the observed reversion frequency at the *am3* codon (T.A.K. and L.A.L., unpublished results). Thus, the error rates reported represent a minimum value.

The high error rate observed in copying natural DNA, one mismatch per 1,000 nucleotides polymerised, is nearly one order of magnitude greater than that observed in initial experiments with *E. coli* DNA polymerase I, 1 in 6,000 to 1 in 20,000 (ref. 11 and T.A.K. and L.A.L., unpublished results). However, we have observed certain preparations of AMV DNA polymerase which copy both poly (dA-dT) and Φ X DNA at lower error rates. The different error rates among different preparations of AMV DNA polymerase might be due to the degree of phosphorylation of the enzyme itself. A protein kinase has been purified from AMV which is able to phosphorylate AMV DNA polymerase, thereby increasing the rate of catalysis¹⁵. The *sarc* gene, the viral gene responsible for transformation in culture and the induction of sarcomas in animals, has been demonstrated to be a protein kinase¹⁶.

Because DNA polymerases from AMV and other RNA tumour viruses seem to be highly error prone *in vitro*, we may ask whether RNA tumour viruses have a correspondingly high mutation rate *in vivo*. This correspondence would be consistent with the hypothesis of Eigen and Schuster¹⁷ relating error rate of replicating enzymes to genome size. A spontaneous mutation rate in the host-range marker in the RNA tumour virus B77 has been measured to be about 1 per 100 pro-virus replications¹⁸ and a similarly high mutation rate has been observed in the *sarc* gene of other strains of Rous sarcoma virus (B. Mason and D. K. Dube, unpublished results). These high spontaneous *in vivo* mutation rates observed for RNA tumour viruses may in part be caused by the viral DNA polymerase which frequently incorporates mis-matched nucleotides during DNA synthesis. The ability of DNA polymerases to copy DNA templates from any source has led to the hypothesis that DNA polymerases from oncogenic viruses copy host DNA inaccurately, causing mutations and initiating tumorigenesis⁶.

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Ribose methylation and resistance to thiostrepton

THE antibiotic, thiostrepton, produced by *Streptomyces azureus*, inhibits bacterial protein synthesis by binding to 50S ribosomal subunits with 1:1 stoichiometry. Recently¹, we showed that *S. azureus* prevents its own death by methylating 23S ribosomal RNA (rRNA) so that its ribosomes do not bind the drug. Intact ribosomes are not substrates for the methylase and those experiments involved methylation of 'core particles' derived from the (thiostrepton-sensitive) ribosomes of *Streptomyces coelicolor*. Ribosomes can be reconstituted from core particles by adding back the missing 'split proteins'. Presumably, in *S. azureus*, methylation of 23S rRNA occurs before assembly of the 50S ribosomal subunit is completed. Here we report that the 'thiostrepton-resistance' methylase of *S. azureus* is an RNA-ribose methylase which introduces a single methyl group into adenosine in 23S rRNA. This is a novel antibiotic-resistance mechanism.

Crude methylase was prepared from *S. azureus* as previously described¹ and was used to methylate 3 M LiCl core particles from *S. coelicolor* ribosomes together with S-adenosyl[methyl-³H]methionine cofactor. As shown in Fig. 1, ³H-methyl groups were incorporated into at least three different residues in rRNA one of which predominated greatly over the others. Acid hydrolysates of rRNA prepared as in Fig. 1 contain mainly purine

bases, pyrimidine nucleoside monophosphates and ribose phosphate. When the major peak of radioactivity was eluted from chromatograms such as that shown in Fig. 1 and incubated with acid phosphatase, the resultant material was not adsorbed by activated charcoal (thus, it did not contain a purine or pyrimidine base) and did not migrate on high voltage paper electrophoresis (HVPE) at any pH between 2.5 and 10.5. We therefore concluded that the major methylated product visualised in Fig. 1 was 2'-O-methylribose-5'-phosphate and that it had probably been derived from a purine nucleotide in rRNA. The minor methylated products in Fig. 1 were purine bases and we have identified one as N⁶-methyladenine (data not given).

The stoichiometry of methylation of *S. coelicolor* ribosomal core particles by *S. azureus* crude methylase corresponded at saturation to 1.1–1.2 methyl groups incorporated per core (Fig. 2). Because in Fig. 1 ~85% of the ³H-methyl groups incorporated were recovered in the major product (2'-O-methylribose phosphate), we concluded that this material had been produced to the extent of one residue per core particle. In contrast, the more abundant of the minor products (N⁶-methyladenine) occurred on average only once per 7.5–8.0 core particles. As core particles methylated in these conditions completely lost the ability to support thiostrepton binding (after reconstitution with split proteins), we concluded that a causal connection exists between ribose methylation and resistance to the drug. This conclusion was supported by the observation (Fig. 2) that the time courses of methylation and acquisition of resistance to thiostrepton were superimposable. In other experiments (data not given) we confirmed that methylribose phosphate was produced exclusively in 23S rRNA.

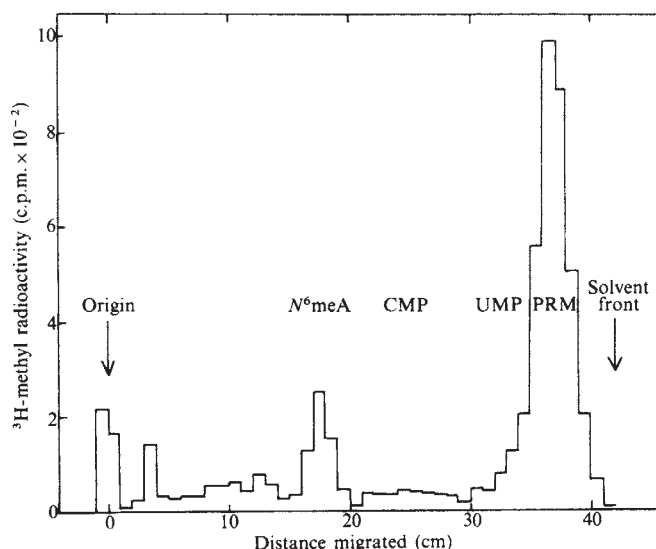


Fig. 1 Chromatography of acid hydrolysates of methylated rRNA from *S. coelicolor*. 500 pmol *S. coelicolor* 3 M LiCl cores (prepared as described previously¹) were incubated at 30 °C for 3 h with crude *S. azureus* methylase¹ together with 25 μCi S-adenosyl[methyl-³H]methionine (500 mCi mmol⁻¹). This was carried out in 1 ml of buffer containing 50 mM HEPES-KOH, pH 7.5 at 20 °C, 7.5 mM MgCl₂, 37.5 mM NH₄Cl, 3 mM mercaptoethanol. Cores were then extracted twice with water-saturated phenol and rRNA was precipitated from the aqueous phase with 2.5 volumes of ethanol at -20 °C for at least 2 h. Precipitation with ethanol was repeated after re-dissolving the RNA in 0.25 M sodium acetate. Hydrolysis of RNA in 1 M HCl was carried out for 60 min in a boiling water-bath and the hydrolysate was applied to Whatman 3 MM paper. Descending chromatography was carried out in solvent containing isopropanol-water-concorder UV light and radioactivity in sample tracks was estimated by liquid-scintillation spectrometry after cutting the tracks into strips 1 cm wide. 'N⁶meA' represents N⁶-methyladenine; 'PRM', 2'-O-methylribose-5'-phosphate.

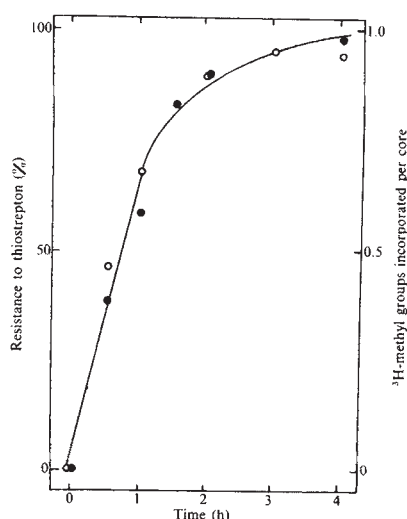


Fig. 2 Time courses of methylation and acquisition of thiostrepton resistance. **A**, Methylation: *S. coelicolor* cores were methylated as in Fig. 1 except that the incubation volume was reduced to 200 μ l. At the times indicated samples (20 μ l) were removed into 2 ml ice-cold 10% (w/v) trichloroacetic acid (TCA) and after 30 min at 0 °C precipitates were collected on glass fibre disks (Whatman GFC). These were washed with 5 $\times$ 4 ml of 5% (w/v) TCA and dried before estimation of their radioactivity by liquid-scintillation spectrometry. Counting efficiency was approximately 26% so that 1 pmol of ^3H -methyl radioactivity gave 285 c.p.m. **B**, Thiostrepton resistance ($\circ$): *S. coelicolor* cores were methylated as in **A** above except that non-radioactive *S*-adenosyl-methionine (0.5 mM final concentration) was used. Samples (20 μ l) containing 16 pmol of cores were removed as indicated and were incubated with a twofold excess of *S. coelicolor* 3 M LiCl split proteins (prepared as previously described¹) and then with ^{35}S -thiostrepton (35 pmol) as described earlier¹. Binding of ^{35}S -thiostrepton to reconstituted particles was assayed by gel filtration¹. The specific radioactivity of ^{35}S -thiostrepton was 1,053 c.p.m. pmol⁻¹. Results have been normalised. Cores do not bind ^{35}S -thiostrepton and reconstituted non-methylated particles do so with approximately 1:1 stoichiometry (this latter level of drug binding was taken as 100%).

The major methylated nucleoside produced in 23S rRNA by the action of *S. azureus* methylase was identified by a combination of paper chromatography and HVPE following digestion of the RNA with snake venom phosphodiesterase and bacterial alkaline phosphatase². Thus, the methylated nucleoside migrated as a single peak of radioactivity during paper chromatography in butanol-ammonia solvent (Fig. 3a). This procedure also served to deproteinise the nucleoside preparation so that, following elution from the paper and lyophilisation, it could be further analysed by HVPE. As shown in Fig. 3b, the methylated nucleoside migrated with adenosine and cytidine at pH 2.5 and was well separated from guanosine and uridine, which are uncharged at this pH. At pH 3.5 (data not given) cytidine migrated well ahead of adenosine and the methylated nucleoside co-migrated with the latter. Also, the methylated nucleoside remained at the origin during HVPE at pH 9.3 in borate buffer (data not given). In these latter conditions nucleosides containing non-methylated ribose migrate towards the anode as borate complexes. Further evidence that the methylated nucleoside contained 2'-O-methylribose was obtained by HVPE at pH 2.5 after heating in a boiling water bath for 20 min with 1M HCl (Fig. 3c). All the radioactivity which had previously migrated as a cation with adenosine (Fig. 3b) was now found to remain near the origin with uncharged markers (for example, ribose and deoxyribose). Accordingly, and because there was no reason to suppose that the adenine ring was modified in addition to ribose, we concluded that the methylated nucleoside under investigation was 2'-O-methyladenosine. (For the chromatographic properties of various base-modified adenosines see ref. 4.)

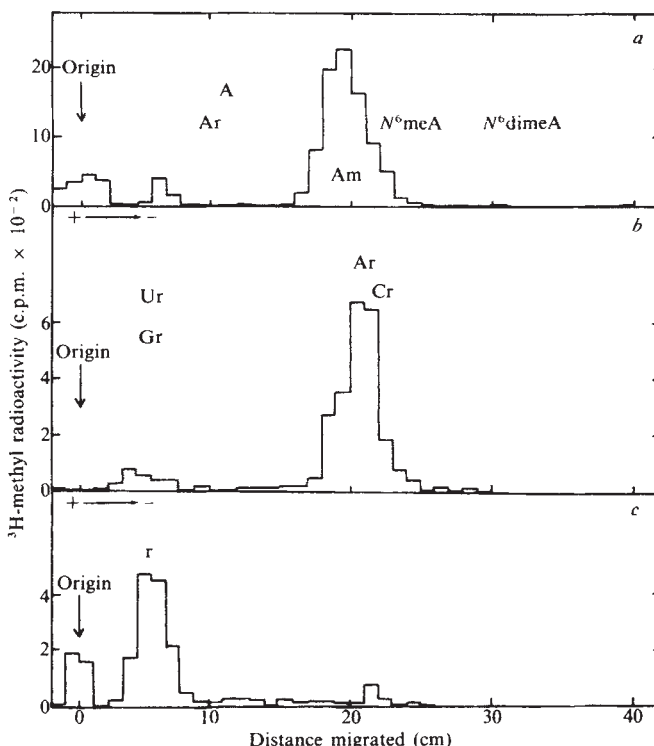


Fig. 3 Identification of methylated nucleoside. *S. coelicolor* 3 M LiCl cores (500 pmol) were methylated with the crude *S. azureus* methylase and *S*-adenosyl[methyl- ^3H]methionine and then RNA extracted as described in Fig. 1. RNA was dissolved in 25 ml 10 mM Tris-HCl, pH 8.0, and incubated for 18 h at 37 °C with 2 mg crude snake venom phosphodiesterase (from *Crotalus atrox*) and at the same time 1.3 mg of bacterial alkaline phosphatase². Finally, the reaction mixture was lyophilised. **a**, The lyophilised digest, still containing protein, was dissolved in a minimum volume of water and applied to Whatman 3 MM paper for descending chromatography in solvent containing butanol:water:concentrated ammonia solution in the proportions 86:14:5. Routinely, such chromatograms ran 50 cm in 15–18 h (the full length of the paper) and were removed for drying after 20–24 h. Markers were detected under UV light and radioactivity in the sample tracks was estimated by liquid-scintillation spectrometry after cutting the tracks into strips 1 cm wide. Material corresponding to the single peak of radioactivity running on these chromatograms was eluted, lyophilised and taken up in water. This preparation of methylated nucleoside was further analysed by HVPE as described below and in the text. **b**, Methylated nucleoside was applied to Whatman 3 MM paper and subjected to HVPE using 50 mM glycine-HCl buffer, pH 2.5. Electrophoresis was for 45 min at 4,000 V in a flat-bed water-cooled apparatus very similar to one described elsewhere³. Detection of markers and of radioactivity in the sample track was carried out as above. **c**, Methylated nucleoside was hydrolysed by addition of an equal volume of 2 M HCl followed by immersion in a boiling water bath for 20 min. The hydrolysate was then subjected to HVPE at pH 2.5 as in **b**. Ribose and deoxyribose markers were detected by standard staining techniques. Following electrophoresis, uncharged markers such as ribose, deoxyribose and (at pH 2.5) uridine were located a short distance from the origin (**b**, **c**) due to endosmotic flow. Abbreviations: Ar and similar = nucleosides; Am = 2'-O-methyladenosine; N⁶meA = N⁶-methyladenine; N⁶dimeA = N⁶,N⁶-dimethyladenine; r = ribose.

(resistance to macrolide, lincosamide and streptogramin B-type antibiotics), possess 23S rRNA which is over-methylated. In this case, sub-lethal concentrations of erythromycin (a macrolide) induce the synthesis of a plasmid-encoded enzyme which produces approximately two residues of N^6,N^6 -dimethyladenine in 23S rRNA (ref. 6). Recently, there has been evidence to suggest the existence of a similar enzyme in *Streptomyces erythreus*, the organism which produces erythromycin⁷. The only other resistance phenotype known to involve some property of rRNA concerns resistance to viomycin in *Mycobacterium smegmatis*. Mutants of this organism possess lesions in either 16S or 23S rRNA, but the nature of such lesions is unknown⁸.

In conclusion, we note that whereas ribose methylation occurs only rarely in the rRNA of prokaryotes⁹ it occurs much more frequently in the rRNA of eukaryotes¹⁰. It is possible that the different sensitivities of prokaryotic and eukaryotic ribosomes to thiostrepton might ultimately be determined by patterns of ribose methylation in rRNA.

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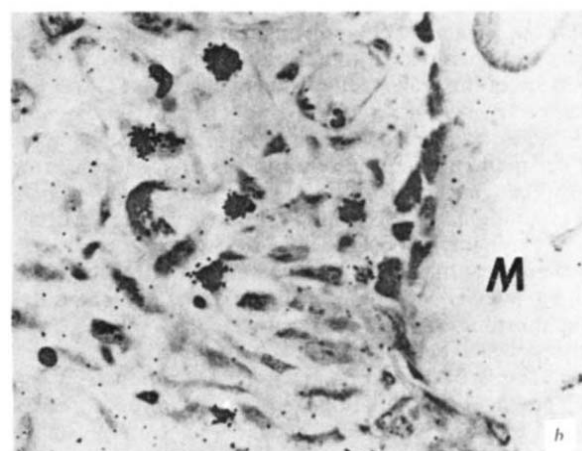
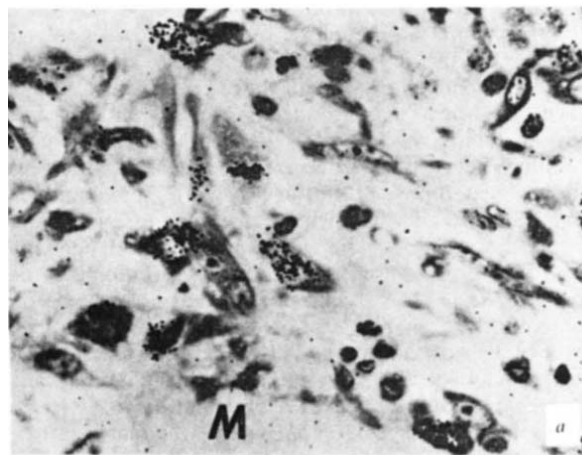


Fig. 1 Photoacoustic absorption spectra of native (a) and denatured (b) lobster shell as a function of modulation frequency ω (indicated with each spectrum) and phase ϕ . The interior spectra (—) were recorded with ϕ selected for maximum discrimination against the surface signal. In this case, decreasing ω allows response from deeper within the sample. The surface region spectra (---) were recorded with ϕ selected for maximum response from the surface. This selection of phase angle restricts the response to the surface region and results in a spectrum which is essentially independent of ω . Square wave modulation used in our spectrometer results in a triangular waveform from a carbon black (soot) sample and a concomitant 90° phase shift from the reference. Thus, the surface spectrum is optimised at $\phi = 90^\circ$ and discriminated against at $\phi = 0^\circ$. These spectra were recorded with a 10 nm spectral bandpass and a 0.03 Hz bandwidth (4-s time constant) at the lock-in output. The signal intensities are only qualitative.

Lobster shell carotenoprotein organisation *in situ* studied by photoacoustic spectroscopy

A SINGLE carotenoid pigment, astaxanthin, bound to specific proteins in a complex quaternary structure, provides shell coloration in the lobster *Homarus americanus*^{1–8}. The absorption of light by these carotenoproteins is uniform over a remarkable spectral range, as evidenced by the reflectance of a blue shell fragment which is low (5–6%) and essentially constant from 400 to 700 nm (ref. 9). This uniform absorption cannot be assigned to any combination of the carotenoprotein complexes which have been isolated. To assess the significance of these chemical species and to make specific inferences about carotenoprotein function *in vivo*, it is essential to measure the absorption spectrum *in situ*¹⁰. Several astaxanthin–protein complexes which absorb in the visible region have been isolated and spectroscopically characterised^{1–9,11}. These include α - and γ -crustacyanin with absorption maxima at 632 and 625 nm, respectively, and a second complex called the yellow protein^{7,11} which absorbs maximally at 410 nm. An early *in situ* absorption measurement of a carapace sample taken immediately after ecdysis and before calcification has been reported by Cheesman *et al.*⁴. However, the absence of structure in the reflectance measurements⁹ from a mature, opaque carapace suggests that conventional spectroscopy can yield little information about absorption maxima *in situ*. By using a new spectroscopic technique, photoacoustic spectroscopy (PAS), we have obtained spectra of a native shell fragment and here report the existence of a near continuum of astaxanthin absorption energies which span the visible spectral region. Furthermore, these pigments are in an anisotropic distribution within the endocuticle, that is, those absorbing in the blue are near the surface whereas those absorbing in the red are in the interior.

PAS is ideally suited for measuring absorption maxima of opaque materials, and as it depends on thermal as well as optical properties of the sample, provides different information from reflectance measurements. It involves the measurement of heat produced as an excited species relaxes by a nonradiative path^{12–18}. The exciting light is chopped at a suitable frequency and the resulting modulated heat flow is detected as pressure fluctuations using a microphone and a lock-in amplifier. As the exciting light is scanned, a spectrum similar to the absorption spectrum is obtained in which the response is proportional to both the absorption cross section and the thermal diffusivity of the sample. It is convenient to specify a thermal diffusion length¹³, $\mu = (2\alpha/\omega)^{1/2}$, where α is the thermal diffusivity and ω the chopping frequency. The definition of μ is such that light which penetrates the sample deeper than μ before absorption does not contribute to the response, as the periodicity is damped out

before the heat diffuses to the surface. Additionally, the phase (ϕ) of the signal at the phase-sensitive detector is critically dependent on the thermal diffusion time, as phase shifts are induced by the time required for thermal diffusion to the sample surface¹⁵⁻¹⁷. Spectral band shapes are influenced by several factors including wavelength-dependent relaxation pathways, saturation effects¹⁴ and, in layered samples, the filter effect of pigments located near the surface. Nevertheless, band maxima from PAS agree very well with measurements by conventional absorption techniques, and the spectrum is independent of ω and ϕ for an isotropic sample, provided saturation is avoided¹⁴. A key feature of PAS is that both ϕ and μ are experimental parameters that can be chosen to optimise the response from different levels of the sample^{16,17}. In certain cases the pigment locations in anisotropic (layered) samples can be determined¹⁵.

The spectra presented here are of a small sample ($\sim 4 \text{ mm}^2$) of uniform deep blue-green colour which was removed from the carapace of a live, mature lobster and used without further preparation. Samples removed from regions of carapace of similar appearance yield indistinguishable results, and samples from regions of obviously different coloration, such as the yellow underside of the claw, yield distinctively different spectra.

Figure 1a presents a series of spectra obtained at different modulation frequencies and phases. It is immediately apparent that the pigment distribution in the carapace is anisotropic so that by changing ω and ϕ one gains access to the different classes (layers) of pigments. Beginning with the surface region, absorption in the near UV by the epicuticle (which consists of wax, polyphenols and various lipoproteins^{4,19}) is evident, together with a band at $\sim 425 \text{ nm}$ which is assigned to the outermost carotenoprotein in the endocuticle. The spectra from the interior of the endocuticle demonstrate a remarkable bathochromic shift in the carotenoprotein absorption, from ~ 450 to $\sim 625 \text{ nm}$, as the modulation frequency is lowered from 326 to 26 Hz. The exception to this trend is the band at 390 nm which is clearly evident in the 26 Hz spectrum and not present in the spectrum from the surface region.

Figure 1b presents the results of a similar study on the shell fragment after denaturation of the carotenoproteins by boiling. The surface region spectrum shows that although the absorption of the epicuticle remains unchanged, the carotenoprotein band at $\sim 425 \text{ nm}$ is shifted to $\sim 450 \text{ nm}$. Only a small change in the interior spectrum is observed as ω is changed. The absorption in the red has disappeared and there is a general appearance of absorption strength in the region 450–560 nm.

We propose that the progressive bathochromic shift of the pigment absorption as a function of depth in the native endocuticle is due to highly specific protein-protein, pigment-protein and pigment-pigment interactions. If this is correct, then it is expected that denaturation would tend to convert all the chromophores to a common form. Indeed, this is the case, and thus the observation of the expected spectra from the denatured sample strongly supports the assignment of an anisotropic distribution of bathochromically shifted pigments in the native endocuticle.

The assignment of bands observed *in situ* to crustacyanins which have been isolated and purified is not straightforward. The spectra presented here, as well as the reflectance spectrum, are characteristic of an essentially continuous shift in the absorption maximum of the crustacyanins. This shift is produced by the interactions described above, which are expected to be a part of the ultrastructure of the endocuticle. Thus, it may not be possible to isolate discrete chemical species corresponding to each absorption. In the case of the denatured shell, the range of absorption maxima between 450 and 560 nm is characteristic of β -crustacyanin and other products of the denaturation of α -crustacyanin^{2,7,8}.

Astaxanthin aggregates, which have been described in terms of exciton states^{11,20}, absorb maximally at 390 nm. This corresponds exactly to the 390 nm band in the native shell and suggests the assignment of this band to the yellow protein *in situ* in which exciton interactions have also been found, although

after isolation and purification, absorption shifts to 410 nm (refs 2,7,9). Assuming a simple exciton model^{21,22} for this species, a monomer absorption maximum at 490 nm and the allowed exciton state at 390 nm, one calculates the corresponding bathochromically shifted and less allowed exciton state to be at 660 nm. Indeed, the absorption spectrum of purified yellow protein² clearly shows absorption by the less allowed transition in this region. Thus, the anisotropic distribution of bathochromically shifted excited states is such that the lowest energy level, the bathochromically shifted exciton state, is in the interior of the endocuticle. This assignment of an interior location to the yellow protein is in contrast to the recent suggestion based on resonance Raman scattering that it is at the shell surface⁹.

A quantitative description of the location of the various carotenoproteins may be obtained from measurements of the phase shift as a function of ω , provided α is known^{16,17}. In the present study, a qualitative estimation of depth is given by μ which ranges from 10 μm at 326 Hz to 35 μm at 26 Hz, if α is taken as $0.001 \text{ cm}^2 \text{ s}^{-1}$. This value of α is typical of biological materials²³ similar to chitin and yields values of μ in excellent agreement with the observed thickness of the epicuticle and depth of the pigmented zone¹⁹.

Although the work described here is structural in that it reports only the detection of a variety of carotenoprotein species and their organisation in the lobster shell, several interpretations are possible. The anisotropic arrangement of the pigments may not serve any photobiological role and may simply be a consequence of the ordered synthesis of the pigmented zone during moulting. Functions such as cryptic coloration may or may not be a property of the distribution of the pigments in layers. However, two possible functions which are dependent on pigment arrangement may be suggested. First, with different receptors serving specific layers of pigment a form of colour discrimination is obtained which could be important in a variety of photobiological processes. Second, pigments arranged with the lowest excited state in the interior could function as an antenna which absorbs light throughout the visible range and transfers excitation energy vectorially to the interior of the endocuticle where receptor sites may be located.

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